THE PROCEEDINGS OF THE PHYSICAL SOCIETY

Section A

VOL. 62, PART 9 1 September 1949 No. 357 A

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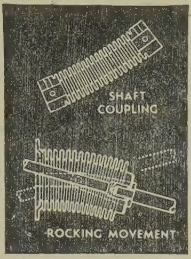

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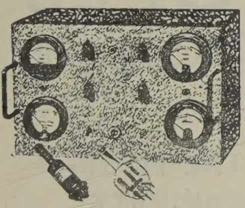

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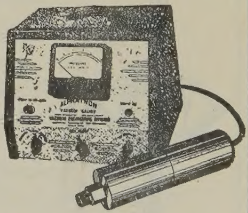

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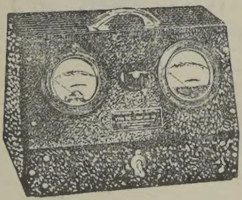

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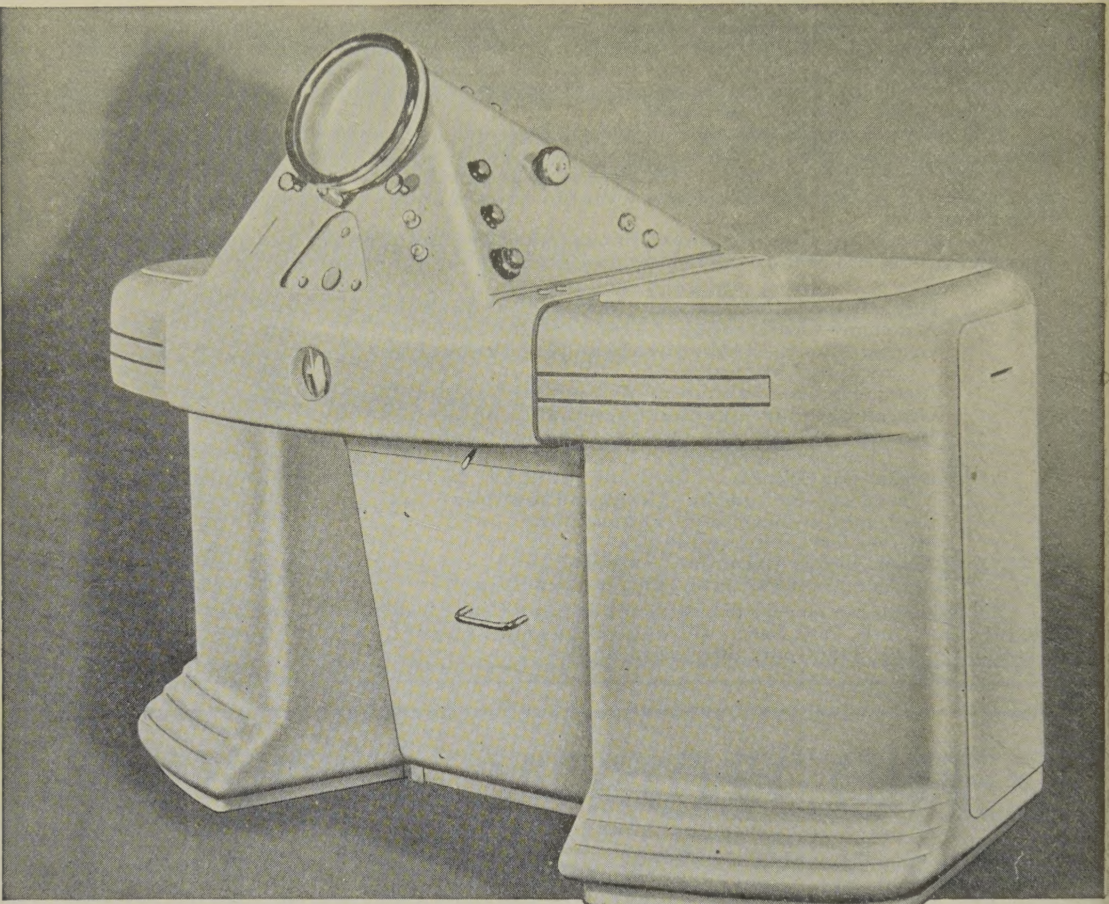

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Section A

VOL. 62, PART 9 1 September 1949 No. 357 A

The Physical Basis of Life*

BY J. D. BERNAL†

Birkbeck College, London

32nd Guthrie Lecture, delivered 21*st November* 1947; *MS. received* 18*th May* 1949

Mr. Chairman, Ladies and Gentlemen,

I feel deeply honoured at being asked to join the very distinguished series of Guthrie lecturers. I have chosen a subject which at first sight seems rather far removed from the work of Professor Guthrie. However, physics has changed much since his time, and even then the foundations were being laid for its transition towards biology. I only know two persons who were students of Professor Guthrie, men of almost completely contrasting temperaments, H. G. Wells and Professor Tutton. The first, as a man of imagination and of qualitative views, did not take kindly to the intensely practical nature of Professor Guthrie's physics course, and has left an amusing account of it in his *Experiment in Autobiography*—"... . Now when I came into the physics laboratory I was given a blowpipe, a piece of glass tubing, a slab of wood which required planing and some bits of paper and brass, and I was told I had to make a barometer. So instead of a student I became an amateur glass worker and carpenter.

"After breaking a fair amount of glass and burning my fingers severely several times, I succeeded in sealing a yard's length tube, bending it, opening out the other end, tacking it on to the plank, filling it with mercury, attaching a scale to it and producing the most inelegant and untruthful barometer the world has ever seen. In the course of some days of heated and uncongenial effort, I had learnt nothing about the barometer, atmospheric pressure, or the science of physics that I had not known thoroughly before I left Midhurst, unless it was the blistering truth that glass can still be intensely hot after it has ceased to glow red.

---

* The title of this lecture is an unconscious echo of the once celebrated lecture of T. H. Huxley (1901) delivered in Edinburgh in 1868. I did not read that lecture till long after I gave this one, but it is interesting to note the changes that eighty years have brought since it was first clearly stated that life had *one* physical basis. Huxley had a word for it—Protoplasm. He was concerned with stressing that it was functionally, formally and substantially the same over the whole range of living things. In function all organisms showed metabolism, movement, growth and reproduction; in form they were all composed of nucleated cells; in substance they were all made of protein, a compound of carbon, hydrogen, oxygen and nitrogen. Not only was life one, but it was linked materially with the non-living, by the capacity of converting inorganic gases and minerals into protoplasm.

The latter half of the lecture was spent in defending the thesis of the former from the charge of rank materialism on the grounds that we did not know any more what matter was than what spirit was, and it was reasonable to accept our ultimate ignorance, however laudable our efforts at understanding. This was the basis of his famous attitude of *Agnosticism.*

The positive part of his lecture stands as firm today as it did when he first put it forward. What is curious, however, is that he nowhere attempted, great evolutionist as he was, to explain the unity of all life in terms of a common history. The developments of physiology and biochemistry have filled in the details of Huxley's picture but they add nothing fundamentally new to it unless they leave the field of describing structures and mechanism and enter into that of searching for origins. It is here, if anywhere, that lies the excuse for making a new attempt at understanding the physical basis of life.

† This article will also appear in *Proc. Phys. Soc.* B for October.

"I was then given a slip of glass on which to etch a millimetre scale with fluorine. Never had millimetre intervals greater individuality than I gave to mine. Again I added nothing to my knowledge—and I stained my only pair of trousers badly with acid."

Professor Tutton, on the other hand, one of the most precise of physicists and crystallographers, rather overdid it in the other direction. Apparently the barometer he produced was not only perfect in every detail but was varnished to boot, and legend has it that Professor Guthrie proceeded to smash it up on the grounds that he was training physicists and not instrument makers.

In one particular respect Professor Guthrie's work does touch the subject of this lecture. He was one of the first to examine the properties of the cryohydrates, those salts in which a large amount of water is held in fixed positions similar to those they occupy in ice. As I shall show, cryohydrates are a significant feature in the crystallography of the proteins and may have had an important part to play in the origin of large molecules adsorbed on clay particles.

I have chosen a very general subject for this lecture mainly because, owing to the events of recent years, I have been unemployed as an experimental physicist for longer than I like to think, and have not been long enough back at work to be able to produce new material on the detailed study of any physical field. Instead, I want to discuss in a general way one of the aspects of physics that is coming more and more into prominence at this time—its relation to the processes of life. At present there are two really exciting fields in physics: one of them is at the outer edge of physics—nuclear physics, and the even more short-period and energetic processes of cosmic rays—and the other at the inner edge—where physics touches biology, a field in which the interest lies in complexity rather than in intrinsic energy. In saying this I am treating physics in its modern and more extensive connotation as including chemistry, because, although chemical manipulation and the logical structural analyses of the older chemistry still remain, and the direct study of material substances and transformations will continue as a source of knowledge, chemistry and physics are now embedded in one common theory and are in future bound to become one integral discipline. Accordingly biophysics and biochemistry, to make any sense, must be considered together.

What I am trying to put before you is the first crude attempt at stating the problems of the origin and function of life from a physical standpoint. In doing so I am fully aware of my inadequacy. It is probable that even a formulation of this problem is beyond the reach of any one scientist, for such a scientist would have to be at the same time a competent mathematician, physicist, and experienced organic chemist, he should have a very extensive knowledge of geology, geophysics and geochemistry and, besides all this, be absolutely at home in all biological disciplines. Sooner or later this task will have to be given to groups representing all these faculties and working closely together theoretically as well as experimentally. The most I can do is to indicate some of the fields in which the key problems exist and provide some preliminary guesses as to the direction in which to look for solutions.

The approach I am choosing is the broadest one possible, and deliberately so. I am not here concerned with the task of elucidating this or that physical structure or mechanism underlying particular functions of existing organisms, but rather with the whole range of the phenomena which we commonly call life. But my emphasis will be on one particular aspect of life, on the problem of *origin* rather than that of structure, metabolism and behaviour. I have chosen this emphasis on origin in biological systems because it is far more important than in physical systems. Until recently discussions on the origins of systems were considered in some way improper to science, but now even in physics itself the questions of origins are coming into discussion, as, for example, those of the origin of the nebulae, the solar system and the elements themselves. Most of what I say will not be new, it is largely based on the writings of other workers, notably Haldane (1928), Lwoff (1943), Oparin (1938) and Dauvillier (1947). What I have done is to correlate these different contributions and add some speculations of my own on the actual conditions under which life originated. My aim is by such a broad sketch to bring out in sharp relief the critical points of difficulty, not in order to evade them by pious allusions to mysteries beyond human comprehension, but as a guide to practical research in the future.

The first obvious step in the discussion should be to define the terms. But life is an extremely difficult term to define. In fact Dr. Pirie (1937) in his paper on "The meaninglessness of the terms 'Life' and 'Living'" has practically proved that it is incapable of definition:

"We have now examined destructively the various qualities which might be used to define the word 'life' and we have found that they are individually inadequate for even an approximate definition. There is not space here to discuss what might be done with all the permutations of these qualities ; it may be said however, that combinations of two or three qualities, though they might easily be drawn up to exclude all obviously non-living systems, will also exclude some which are, if not typically living, at least generally included in that category.

"Until a valid definition has been framed it seems prudent to avoid the use of the word 'life' in any discussion about border-line systems and to refrain from saying that certain observations on a system have proved that it is or is not 'alive'."

By taking every quality, such as respiration or movement, usually considered characteristic of life, he is able to show that there are many things called "alive" which do not possess any of them, and equally many possessing some of them that we do not call "alive".

I wish to avoid these troubles by not attempting to give a definition of any particular form of life or living organism, but rather by treating the common word "life" as referring particularly to the totality of processes accessible to our observation on this earth : to what Goldschmidt has called the biosphere, consisting materially of the group of complex organic compounds found almost exclusively in the watery layer on the surface of the earth, the hydrosphere, or in the adjoining regions of the atmosphere or parts of the lithosphere clearly derived from it. If we limit it in this way we can for the moment find one common material characteristic, the presence of protein molecules, and at the same time one common physico-chemical process, the stepwise catalysis of organic compounds carried out practically isothermally by quantum jumps of between 3 and 16 kilocalories, small compared with the usual jumps of 300 in laboratory chemistry.

These terrestrial limitations obviously beg the question of whether there is any more generalized activity that we can call life. Biology in this respect is on a different basis from physics and chemistry in that it deals less with universals and more with contingents. It belongs to the kind of descriptive and interpretative studies we might more properly call " graphies ", including observational astronomy and geography. Whether there are some general characteristics which would apply not only to life on this planet with its very special set of physical conditions, but to life of any kind, is an interesting but so far purely theoretical question. I once discussed it with Einstein, and he concluded that any generalized description of life would have to include many things that we only call life in a somewhat poetical fashion. Any self-subsisting and dynamically stable entity transforming energy from any source, or, as Haldane put it, "any self-perpetuating pattern of chemical reactions ", might be called " alive " in this sense. The value of distinguishing it as an individual system or organism would only exist if the total phenomena persisted for a time appreciably longer than the periods or characteristic times of any internal processes it might contain. In this sense a galaxy or a star is alive, or, on a more terrestrial scale, a flame. Passing to a degree of complication greater than the biological, we might talk of the life of a human culture or civilization. All are characterized by birth, persistence and death.

These considerations, however, are all very abstract. Their only immediate value is to indicate that though there may be a number of kinds of life in this sense, these kinds of life are quite distinct, and each has a very limited sphere of existence. All must satisfy two necessary criteria. The first is that they must be functional, in the sense that the processes that comprise them must have a certain dynamic stability *.

Now this leads us back again to the method of tackling the problem as one at the same time historical and physiological. The actual material structures which build up our terrestrial life, with which we shall be exclusively concerned from now on, are to be considered at the same time under both aspects, those of their function and of their origin. An object such as a galaxy or an apple carries its whole significance only if we consider the stages of its development as well as its instantaneous activity. The ostensible purpose of biology in unravelling the processes that occur in living things is at the same time the elucidation of the necessary stages by which they arrived at their actual structures. Present study throws light on past history and vice versa. Every existing organism is in this sense a fossil. It carries in it by inference all the evidence of its predecessors ;† and this remains the case even if we cannot read it clearly or at all. Naturally the study of the past, which cannot be directly determined, can provide no positive proof to set against immediate observation of the present ; but it can do something equally important. It can give indications as to where to look in the present to find significant things.

The history of science affords many remarkable examples of this process. The chief of them of course is Darwin's statement of natural selection as a mechanism for evolution. Undoubtedly evolution is a hypothesis, natural selection is

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* The meaning of this first criterion has been made much clearer by the work of Prigogine (1947) on the thermodynamics of " open systems ". Such a system can take in and pass out matter as well as energy and is not subject to the second law of thermodynamics, that is, its entropy may remain constant or even decrease, the condition of stability of such a system being that the rate of change of entropy is a minimum; and secondly, they must originate and develop out of some pre-existing system, or, in plain English, they must work and they must have got there in the first place.

† Professor Haldane says " No, it doesn't. Gene segregation is a trick for halving the amount of the ' evidence ' once per metazoan life cycle. It may carry enough evidence." It would have been better perhaps to have phrased the statement conversely as " everything an organism contains is evidence of its predecessors ".

an observational fact: but it was from the hypothesis of evolution, that is, from the need to provide an explanation of present forms of life in terms of older ones, that Darwin was driven to use the natural selection argument which he borrowed from Malthus. As a result he not only put into order the chaos of the accumulating biological observations of his time, but led to the fertile and useful experimental studies of ecology, morphology and heredity.

Darwin himself, curiously enough, was opposed to any generalization of his own methods. In a letter to Sir J. Hooker (see Darwin 1887) he said: "It is mere rubbish thinking at present of the origin of life; one might as well think of the origin of matter." We are now almost in a position to take him at his word. For the origin of matter, at least the origin of the forms in which matter presents itself on this earth, is at last becoming clear. The chemical elements, in their ordered multiplicity, and with their apparently arbitrary and enormous variations of abundance, are seen now as perfectly logical products of a process going on in some primordial hyperstar or concentrated universe some four billion years ago. In fact, the physicists have reconstructed, somewhat provisionally, the temperatures and pressures of that time from the relative abundances of different elements, and in the process have discovered anomalies which would not have been discovered without some such hypothesis as to origin.

This does not mean that we should accept wild hypotheses of the origin of life or of matter which simply conceal ignorance, but rather that we should attempt almost from the outset to produce careful and logical sequences in which we can hope to demonstrate that certain stages must have preceded certain others, and from these partial sequences gradually build up one coherent history. There are bound to be gaps where this cannot be done, but until the process is attempted these gaps cannot be located, nor can the attempt be made to fill them. The process is not dissimilar, though on a vastly greater scale, to the attempts of the geologists two hundred years ago to determine the history of the crust of our globe, which, once theological preoccupations had been overcome, led to the ordered logical sequences of Hutton and Lyell.

There are a certain number of general considerations which we can employ in building these sequences. In the first place we may know that we can expect gaps where processes are particularly rapid. In any sequence which passes through a number of phases the occurrence of any phase will be proportional to its duration. Unstable elements are the least abundant. Rapidly evolving forms leave the fewest fossils. What we observe easily are the stable stages, and they may form a very small fraction of the whole process, giving a much greater impression of discontinuity than really exists. Life as we know it consists of only a relatively small number of kinds of simple molecules—sugars, amino acids, purines—molecules out of which are composed the far more complicated macro-molecules of the proteins and nucleic acids and microscopically visible structures such as membranes and fibres. We now know, particularly through the use of tracer elements, that there exist as well a large number of intermediate molecules which are changed so quickly that they cannot be observed by ordinary methods. But we may further infer that in the history of life there have been formed a still larger number of molecules that have played a decisive rôle in its chemical evolution, but which can be reconstructed now only from the traces they have left in the structure of existing molecules.

Another very general consideration for all developing systems is the way in which processes occurring for the first time react on those already established. One of the perennial arguments in biology has been the question of spontaneous generation. Ceaselessly put forward to explain otherwise obscure phenomena, it has always been disproved whenever careful work has been carried out, and we are apt to think that Pasteur has settled it for all time. But those who demonstrated that spontaneous generation did not normally occur under existing conditions usually went further and claimed that they had also proved that it could never have occurred at all by natural means, and, therefore, only substituted an original miracle for a series of continuing ones (see Engels 1940). Now one obvious weakness of this argument is that the conditions in which life originated do not exist any more on this earth. It is not necessary, to prove this, to assume any gross change in the external physical conditions, surface temperature or solar energy, etc. It is sufficient to know, as Haldane (1928) has pointed out, that the very existence of life itself radically changed the initial conditions, principally by producing molecular oxygen. Similarly, at almost every stage in the evolution of life, successful new forms interfered with all that had gone before. There is no guarantee, of course, that life itself will not so interfere with the universe into which it has intruded itself as to wipe itself out. All it has done so far is to block possibilities of development other than those actually taken. This implies, of course, a potentiality for life much more extensive than the actuality.

There is a definite and limited range of potentiality fixed at a lower level of complication by the physical and chemical properties of molecules. This corresponds to what Henderson spoke of in his remarkable book of forty years ago, *The Fitness of the Environment*. He pointed out that the peculiar properties of water and carbon dioxide, for instance, were as suitable for life as life was adapted to them. But though life in its terrestrial form depended on these properties, we now see that this is about equivalent to saying that life depends on the laws of arithmetic or geometry, because the peculiar properties of water and carbon dioxide are implicit in the number of electrons contained in their atoms, and that the abundance of atoms with these numbers and their presence on the surface of the earth follows logically from their nuclear composition. When these interactions, however, become as complicated as they have done in life, we can no longer be sure that the only absolutely possible track has been taken. Indeed, from the very variety of life on this earth, in past as in present times, we know what a great range of possibility there has been and what apparently small accidents, some of them outside the range of life altogether, such as those of geography or climate, have determined the success or failure of this or that form.

We can if we like call these possibilities the Aristotelian entelechies, and this can be done without any mystical invocations. Any arrangement of atoms or molecules necessarily carries with it complex possibilities of order and function, and those are *immanent* in the structure of the molecules themselves. Which appear and which do not appear are, however, immanent not in any particular combinations themselves but in the total play of universal forces, and, therefore, for the consideration of the system, may be considered as *contingent* or accidental. It is a convenience for us to separate the immanent and contingent elements, attributing the first to science and the second to cosmic history, but their interaction must always be taken into account. Deepening our knowledge of the behaviour of material systems helps us to detect and to understand historical changes. Conversely, knowledge of the origin and history of life gives a clue to the understanding of biochemistry and microbiology.

The new interest in the physical nature of biological systems has coincided necessarily with the development of new physical tools, theoretical and practical. Of the former, by far the most important is the development of the quantum theory and its extension to cover at least in principle the theories of chemistry and to give precious indications of the far more complex phenomena of biochemistry and biophysics. Without exaggeration it can be said that the conception of quantum energy changes in chemical reactions is the most illuminating and the most effective new idea in modern biology. We begin to see now that the material aspects of a living system are but the struts and levers of a machine, the particular function of which is to effect energy interchanges, and that growth and assimilation are but means of achieving a metabolism consisting of enzyme promoted energy changes. Thus in a very physical sense process takes precedence over structure. The developments of other aspects of physical science also have theoretical applications to biology, particularly the theory of solids and liquids, which may help to explain the apparently mysterious activities which go on inside living cells. On the other side our new knowledge of soil science and of geochemistry, as well as that of physical geography and oceanography, throw light on the medium from which life evolved and which still supports it.

With these advances in understanding has come a much greater armoury of experimental methods. On every level of organization, both of structure and of energy change, new physical and chemical instruments are offering the biologist a range of opportunity unequalled since the first use of the microscope and of the balance in the seventeenth century. On the chemical level the introduction of radioactive tracers is likely so to revolutionize the unravelling of reactions in both organic and biochemistry as to make what we think we know now obsolete in a matter of a few years. For the determination of molecular structure, the use of spectroscopy, particularly infra-red spectroscopy, and of X-ray crystallography combined, gives us a picture of atomic arrangements limited in its accuracy and extent only by the number of workers in the field and the slowness of the computations involved ; and this limitation itself is likely to be removed in a few years by the further development of electronic calculating machines which will enable some years' work to be done in a few hours. Chemical methods themselves have enormously increased their range and accuracy, owing to the development of industrial polymer chemistry. Differential chromatography, which enables the simple and complex constituents of proteins and other organic substances to be analysed and labelled with ease and certainty, provides a bridge between the chemistry and the physics of large molecules.

It is, however, in the intermediate zone, the zone between the chemical molecule of some tens of Ångströms in dimensions, and the old limit of microscopy at 2000 Ångströms, that the most significant advances have been made. Indirect methods of great beauty and interest we have had for some time—the use of the ultra-centrifuge by Svedberg, of electrophoresis by Tiselius, of viscosity by Staudinger, and more recently of light scattering by Doty. These provided the first really quantitative picture of the approximate size and shape of the large molecules of biological and industrial origin. Now, however, we have far more precise and accurate methods. X-ray analysis can be extended to deal with molecules of hundreds of Ångströms in dimensions, and although it cannot as yet give the precise details of their atomic constitution, it has already led to significant information as to their general nature, particularly of their constancy under different physical conditions.

Far more spectacular in recent years, however, has been the development of the electron microscope, making visible in an immediately understandable form structures from those containing a few score atoms to the limits of microscopic vision and beyond. We are certainly now in a Galilean phase of observational biology. Until the advent of the electron microscope, a great blank existed between the knowledge of atomic combinations provided by chemistry and that of histological structures observable with the microscope. This gap was filled with the mystic word "colloid", which served to explain the very real but very obscure properties depending on the existence of structures of magnitudes between ten and ten thousand Ångströms. Now the colloid world is open for inspection, and the term itself will probably vanish or acquire a precise and limited meaning. The coming of the electron microscope, as has often happened in the history of science, has stirred the optical microscopists into exertions which their sole possession of the field did not provoke. The microscope had, in fact, stood virtually still between 1880 and 1940, but now, with the rival in the field, it was found that it could be improved in many ways, and the new phase microscopes, and the ultra-violet, infra-red, polarizing and reflecting microscopes provide an armoury which, though they cannot rival the electron microscope in resolving power, have, in the hands of such workers as Casperssen, shown a power of chemical interpretation of structure greater than any other method, and having the additional enormous advantage that they can be used on living material. With all these new methods simultaneously available it is not surprising that the present picture of biological research should be one of exciting but somewhat disordered advance—an advance so rapid that attempts to assess the position will be out of date almost as soon as, if not before, they are written.

In the account which follows an attempt is made to present the main outlines and the critical stages in the development of life from its inorganic origins. It is based essentially on two kinds of data—the geochemistry and physico-chemistry of the cooling planet, and the organic chemical composition common to all existing living organisms. Such an attempt reveals at once the large gaps that still exist, but it also reveals the lack of perfectly feasible research which is bound to help to reduce these gaps and to bring out others that may now be unsuspected.

The process is one which we can imagine as taking the form of a play divided into a prologue and three acts. The prologue introduces the scene on the surface of the primitive earth, and the first group of actors of an entirely inorganic kind which must start the play. The first act deals with the accumulation of chemical substances and the appearance of a stable process of conversion between them, which we call life ; the second with the almost equally important stabilization of that process and its freeing from energy dependence on anything but sunlight. It is a stage of further synthesis and of the appearance of molecular oxygen and respiration. The third act is that of the development of specific organisms, cells, animals and plants from these beginnings. All we have hitherto studied in biology is really summed up in the last few lines of this act, and from this and the stage set we have to infer the rest of the play.

Our knowledge of the nature of the surface of the primitive earth is derived from the spectroscopic evidence as to the abundance of the elements in the stars, and the much more meagre information we have about them in the atmospheres of the planets on the one hand, and on the other the knowledge of geochemistry and geophysics. This field of study has been entirely transformed in recent years through the application of x-ray crystallography in the hands particularly of the Braggs and Goldschmidt. In the latter's great work, *Verteilungsgesetz der Elemente* (1923–37), are traced the processes by which the primitively mixed elements of the original solar filament sorted themselves out in the first place under simple gravity, and at the latter stages by crystallization according to the laws of ionic combination, which he discovered and Pauling later refined. It is still an open question whether the first sorting process was ever complete, whether the still liquid interior of the earth consists of iron or of unconsolidated solar matter, mostly of hydrogen, as Kuhn (1946) has recently proposed.

The outer layers, however, in their melted form, must have contained in addition to silicates—predominantly basic silicates—much water and carbonate in solution, the whole being originally surrounded by an atmosphere of hydrogen and hydrides, CH_4, NH_3, H_2S, and H_2O though in far less quantities than now. This original atmosphere must have been modified in two particular ways. The actual crystallization of the crust must have forced into the atmosphere vast quantities of water vapour and carbon dioxide. At the same time hydrogen was being steadily lost at the top of the atmosphere, the gravitational pull not being strong enough to hold it. The result is bound to have been a steady oxidation, methane turning to CO_2 and water through intermediate compounds such as aldehydes, alcohols and acids which may have had a rôle in the formation of life ; ammonia oxidized to nitrogen and H_2S to sulphur. Further oxidation seems most unlikely at the pre-organic stage. Some must have occurred at the very top of the atmosphere in the dissociation of water to hydroxyl and free hydrogen, but the small amount of hydroxyl formed would be used up on oxidizing compounds in the lower atmosphere, and there remained an enormous residue of reducing material in the ferrous iron of the then exposed primitive rocks.

By the time the earth had cooled sufficiently for the water to condense, the atmosphere may have been largely one of nitrogen with a gradually decreasing concentration of carbon dioxide as its partition coefficient in the cooling sea became less and less. The seas would contain primarily ammonia, carbon dioxide and hydrogen sulphide in solution. Whether they also contained salt in anything like the present concentration is still an open question, though there are enough minerals containing chloride to provide for present concentration if sufficient rock had been worked over in the course of geological history by weathering processes ; but on the other hand it is possible that vapourized halides formed parts of the original atmosphere, came down molten, solidified and were dissolved in the primitive seas (Dauvillier 1947).

The surface of the world at this stage must not have been very different from what it is now, except for the bareness of the rocks and consequently greater speed of weathering. We still do not understand the mechanism of continent and mountain formation, but there is no reason to believe that it was dependent on organic processes. We may therefore assume most of the geographical features which we now observe, with the exception of coral islands, though inorganic calcium carbonate precipitation may have occurred. In particular there must have been, as now, extensive areas of mud in deltas and on continental shelves, some of which would be exposed to the tides. Correspondingly on the land a kind of soil would be formed wherever the run-off was not sufficient to remove weathered material or where it was deposited in rivers, and this soil would also contain clay.

At first sight there might be no reason why the world should not have continued in this state indefinitely, but there was an active agent operating at that time which is no longer in operation, namely the influx of solar radiation out to the far ultra-violet of 2000 A. or less. If we put to a physical chemist the problem of the reactions occurring in a weak solution of ammonium carbonate and sulphide under such radiation, he would agree that although it was not possible without experiment to determine exactly what compounds would be formed, a process of polymerization and condensation leading to the formation of nitrogenous organic compounds such as the amino acids is almost certain, and would proceed until an equilibrium was reached where breakdown was equal to formation. It would be of the utmost importance to make these vague remarks more precise, and researches in ultra-violet photosynthesis would certainly be of interest, and might be of practical use.

Equilibrium, moreover, could be reached in two ways. One would be the straight photochemical process of absorption and emission ; the other might well be a dark breakdown through a number of intermediate compounds liberating energies corresponding to smaller quantum steps and longer wavelengths. Life from the purely physico-chemical point of view is simply a denotation of the complex mechanism of this latter process. We may give a schematic picture of this in the form of a graph showing purely qualitatively the effective utilization of sunlight at different periods of world history. At first the absorption is determined only by the original constituents of CO_2 and ammonia. Later the more complex forms give rise to greater absorption. When life appears, and these forms are broken down, the absorption decreases. The subsequent hypothetical changes due to the appearance of organic photosynthesis and respiration will be discussed later.

The stage is now set for the appearance of life itself. In Moleschott's classic phase, "It is woven out of air by light". It is here that organic chemistry first begins. Condensations and dehydrogenations are bound to lead to increasingly unsaturated substances, and ultimately to simple and possibly even to condensed ring structures, almost certainly containing nitrogen, such as the pyrimidines and purines. The appearance of such molecules makes possible still further syntheses. The primary difficulty, however, of imagining processes going thus far is the extreme dilution of the system if it is supposed to take place in the free ocean. The concentration of products is an absolute necessity for any further evolution. One method of concentration would of course take place in lagoons and pools which are bound to have fringed all early coastlines, produced by the same physical factors of wind and wave that produce them today. It has occurred to me, however, that a much more favourable condition for concentration, and one which must certainly have taken place on a very large scale, is that of adsorption in fine clay deposits, marine and fresh water. Our recent knowledge of the structures of clays has shown what an enormous rôle they still play in living processes. There is probably today more living matter that is protein in the soil and in the estuarine and sea-bed clays than above the surface or in the waters. Now the effective part of this fine-grained clay is known, particularly through the electron microscope studies of Hast (1947), to consist of what might reasonably be called clay molecules, single layers of aluminium silicate some ten Ångströms thick and a hundred and forty across, covered on both surfaces with hydroxyl groups and capable of adhering with a larger or smaller number of water molecules into small pseudo-crystals like piles of coins. Such a small clay particle has an enormous effective adsorptive surface. It has already been shown, particularly by MacEwan (1948), that organic chemicals of a wide variety are preferentially adsorbed on such surfaces in a regular way. It is therefore certain that the primary photochemical products would be so adsorbed, and during the movement of the clay might easily be held blocked from further possibly destructive transformations. In this way relatively large concentrations of molecules could be formed.

This formation is impossible nowadays for two reasons: firstly the cutting off of the ultra-violet by the ozone layer, and secondly the almost universal presence of life, which would destroy such molecules if they were formed in any other way. The very absence of life ensures an accumulation of material containing available energy for indefinite periods : the original world was sterile. Now the clay molecules have another property besides adsorption. Small molecules attached to them are not fixed at random, but in definite positions relative not only to the clay but to each other, and held in such positions they can interact and form more complex compounds, especially if energy can be supplied in the form of light. Clays are now one of the most important of industrial catalysts*. Polymerization would take place particularly easily with unsaturated compounds, with relatively free electrons. It is in this way that we may imagine that simpler molecular compounds could be made to undergo complex polymerization, polymerization to such an extent that the macromolecules produced might be able to persist in a colloidal form even without clay, and become catalysts or, as we should now call them, enzymes in their turn.

Clay is not the only material on which adsorption may take place. Quartz is another very active material which, as sand, would occur separately or together with clay. The importance of quartz in the formation of primitive molecules out of which life is constructed may be a crucial one in that quartz is the only common mineral possessing asymmetrical structure, some crystals having a right-handed twist and others a left. The characteristic of molecules occurring in living organisms, first brought out by Pasteur a hundred years ago, is that they are also asymmetric, and it has always been a very great difficulty to explain life originating with such molecules, as normal chemical processes produce right- and left-handed molecules with equal facility. It may be, of course, as Pasteur himself thought, that some general feature in the environment favours one rather than the other type, for example, the rotary polarization of moonlight or the magnetic moment of the earth ; but to me it seems more plausible that the particular twist was given at one time by the preferential adsorption of a pair of asymmetric molecules on quartz, and, as Mills has shown, once one asymmetric isomer was produced, even locally, it would produce a situation in which ultimately only one kind could be formed.

So far we have followed the track from the inorganic world by steps which, though they cannot be indicated in detail for the lack of necessary research, most of which is quite feasible, nevertheless are not only plausible, but, with our present knowledge, inevitable. However, to get any significant information we must look at the other end of the play and try to draw deductions from the inner chemical structure of actual organisms. Now here we are met at the outset with a remarkable set of facts, which call for explanation in terms of origin. The overwhelming majority of living organisms, from the lowest bacteria to trees and men, are all built of a relatively small number (about thirty in all) of types of chemical molecules containing between four and forty atoms in each. Every chemical molecule has its origin in some previous combination, as certainly as every atom of every element. More complex forms, particularly in this or that organism, can be derived from those simple types, and the few cases which have been studied have been shown to be so derived.

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* The mechanism of catalysis and enzyme action is now becoming much clearer. It seems to depend to a very large extent on movement of hydrogen ions or protons for the most mobile of all chemical species. The most illuminating experiment carried out by Turkevich and Smith (1946) on phosphoric acid translocation of double bonds in butenes, using tritium as a tracer, shows that the actual process consists of a simultaneous transfer of a proton from one part of the molecule to the phosphate, and simultaneously one from the phosphate to another part of the molecule, thus effecting a proton transfer from one part of the molecule to the other, and leaving the phosphate without any net change. They point out, however, that most known crystals, either of a metallic kind or of an ionic kind, such as silicates, sulphates etc., can effect such transfers, and it seems highly probable that protein enzymes act in a similar way.

From this it follows that there is only one predominating life, derived from one common chemical basis*. This is exactly the same logic that shows, for example, that all so-called Indo-European languages have a common set of root words, however much they have deviated afterwards. It would be wrong, however, to assume that the common molecules, the amino acids, the sugars and the purines which are the joint stock of existing life are necessarily the first organic chemicals, because there are to be found some aberrant bacteria : the purple and green sulphur bacteria which do not contain some of these molecules, in particular free sugar. It may be that modern life, as we may call it, represents a second stage, and we may have to reconstruct the first stage from these particularly primitive survivals. But all life, including these, contains one group of compounds of a far more complex nature which does seem to be of crucial importance, namely the proteins.

A hundred years ago Engels referred to life as the " mode of action of albuminous or protein substances ", and biochemical advances have only confirmed this dictum and made it more precise. The work of the last fifty years has shown something both of their function and of their structure. So many of the chemical reactions occurring in living systems have been shown to be catalytic processes occurring isothermally on the surface of specific proteins, referred to as enzymes, that it seems fairly safe to assume that all are of this nature and that the proteins are the necessary basis for carrying out the processes that we call life. Now although since the great work of Fischer we know that the proteins consist of various combinations of some twenty amino acids, we still do not know the precise structure of any of them. But we do know that they have a precise structure, and we have reasonable hope of determining it in the not so very distant future. It is perhaps significant that though the number of different proteins may be counted in tens of thousands, this represents an insignificant proportion of the possible combination of twenty amino acids. The most likely explanation is that certain sub-units containing the same amino acids in the same order must occur over and over again.

The work of the physico-chemists, particularly of Svedberg, has shown that active proteins exist in the form of molecules of definite molecular weight, and more recently x-ray structure analysis has shown that they are perfectly definite chemical compounds with identical molecules which persist unchanged through various grades of crystal hydration and into solution. We possess already much information as to the actual molecular arrangement, but unfortunately its full interpretation is a task for the future. The situation at the moment is extremely similar to that of an archaeological expedition that has discovered large quantities of rock inscriptions in an unreadable script. They may not know what the inscriptions mean, but they do know they mean something, and they may reasonably hope to decipher them.

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* This was the major conclusion of T. H. Huxley's *The Physical Basis of Life*, see footnote, p. 537.

The first hints at the deciphering come from the structures of degraded proteins, those used for structural purposes in the animals themselves, such as skin and hair, and those produced by violent chemical action, such as in boiling an egg. Here Astbury (1933) has shown that polymerization must occur to form long chains, and that these chains may be straight or curved or folded in a definite plane. As far as the straight chains are concerned this has been confirmed by the artificial polypeptides synthesized by Woodward giving similar X-ray pictures to the natural ones. More recent work by Perutz (1949), Crowfoot (1941) and their pupils has shown that a similar arrangement must hold in the crystalline proteins, and there seems to be emerging a general picture of the primitive protein molecule, such as that derived by Crowfoot for gramicidin–s, or by Kendrew for myoglobin, indicating the existence of parallel groups of chains making up flat layers of about ten Ångströms thick and from thirty to sixty Ångströms wide. The primitive protein may therefore have a resemblance which is more than coincidental to that of the clay particle. More complex proteins are built by agglutination of the simpler groups. Haemoglobin, for instance, studied in detail by Perutz, seems to consist of four of the myoglobin layers held firmly together. In the crystal the molecules may further be separated by layers of water, just as in the clay particles. The complication goes further with the building up of even larger protein molecules, right up to the size of the viruses whose internally crystalline nature, identical in the solid gel and solution, I was able to establish ten years ago with Bawden, Pirie and Fankuchen (1936).

Now the electron microscope has shown the absolute continuity between the external structure of protein molecules and viruses. I am stating this without any idea of claiming that viruses are a primitive form of life. All the evidence points in the opposite direction. The activity of viruses is absolutely dependent on that of other organisms, and they are probably some of the most complex and sophisticated forms of life, though they seem to lack all the structures and most of the enzymes that normal organisms possess. They would appear to be degenerate parasitic forms, having cut everything down to essential characteristic nucleo-protein, rather than primitive forms evolved independently of higher organisms.

Their significance here is that they illustrate the effects on physical structure of the existence of large regular molecules. Once the size of 100 Ångströms or so is surpassed, new kinds of interactions, imperceptible against the thermal background at smaller sizes, become apparent. These are the long range forces assumed to account for many colloidal phenomena, such as gel formation and coacervation. Tobacco mosaic virus provided the first quantitative pictures of such forces, because it showed that the identical rod-shaped particles of the virus maintained themselves at equilibrium distances which are dependent on the pH and salt concentration. The physical nature of these forces, though almost certainly connected with ionic atmospheres, is still in dispute. Langmuir, Levine, and Verwey and Overbeek (1948) have produced theories which account for them qualitatively, but of their real existence there can be no doubt.

This statement of course does not exclude the possibility that no physical forces exist, but instead that thermodynamic equilibria are satisfied for particles maintaining certain distances from each other. What is important for biology is that such distances can be maintained in media such as those of cells.

The moment, therefore, that macro-molecules of this type are produced they must interact in the liquid and give rise to the physical unity which characterizes the individual living organism. Because of the vagueness of the terms, it is difficult to say whether we should count the origin of life from the first moment that steady interactions occurred between complex molecules in a general medium, or from the point when a section of this medium was separated out, sufficiently large to contain a self-maintaining system of reactions with the medium. We might, if we wish to be precise, qualify the first with the origin of *life*, the second with the origin of *organisms* or living things. The first stage is difficult for us to grasp. The idea of life without living things may seem a contradiction in terms, and yet the evidence, as far as it goes, indicates that such a stage must have occurred because simple molecules must have preceded complex ones, and because definite organisms cannot be formed without the pre-existence of complex molecules*.

For the purposes of understanding the early stages of development of life it is, however, not absolutely necessary to stipulate the colloidal state of the system. For one thing that is becoming increasingly clear of recent years is that the chemical reactions and the modifications of these reactions are the most characteristic features of vital activity. We must therefore infer a long stage of chemical evolution, of what might be called the internal economy of life, long before we need consider the external shape or forms of function of living things. There is work now to be done on the chemical evolution of life more difficult but also far more fundamental and useful than that which Darwin did on the evolution of the higher forms. The key to the understanding of this chemical evolution of life lies in the junction between observational biochemistry on the one hand and quantum theory on the other.

The basic chemical problem of all vital transformations is the achievement of chemical changes in an isothermal medium, by which large amounts of energy can be made use of in the small steps which alone are permissible in such a system. It is found, for instance, that in the linked enzyme systems which occur in all vital chemical transformations, the individual steps are never more than some sixteen calories and are often much less, down to three calories, hardly more than that of a hydrogen bond, whereas the total exchanges of energy may be several hundred calories, equivalent to the complete combustion which, without these enzyme mechanisms, could only occur at high temperatures. In providing these small quantum jumps, the existence of particularly labile inorganic reactions is obviously of the most critical importance. Two of these, on account of their extreme abundance, are known to be specially important—the oxidation and reduction of ferrous to ferric iron, and of sulphydsyl, —SH, to disulphide, —S—S—. Early life, in the absence of atmospheric oxygen, must have proceeded almost entirely by the enzymic utilization of these transformations.

The processes of life have a close analogy to those of a chemical factory, only here, instead of the materials being poured from one reaction vessel to another, the individual molecules diffuse from one enzyme to the next, the rates are fixed, and a number of them circulate in these cycles, making use of a certain fraction of the available energy of the reaction, to reverse the entropy gain. As Schroedinger has said, life consumes not food, but negative entropy, and it is able to do so by the existence of what are effectively solid structures in the protein molecules themselves. It is with the establishment of any one such chain of reactions where complex molecules can be fed in at one end and simple ones liberated at the other, with a net energy gain, that we may imagine that living processes started.

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* I owe this example of such a system to Haldane. The bacterium *Haemophilus caris* cannot synthesize hematin and *H. influenzae* cannot synthesize coenzymes I and II. Neither can grow alone on peptone, but a mixed culture grows well.

We have seen that the complex molecules themselves may have been produced by straight light absorption, but on the other hand the environment did not contain any large sinks of energy such as are provided by the existence of free oxygen molecules in the present atmosphere. The first life processes, therefore, must have been extremely inefficient, similar to those that exist at the moment in anaerobic fermentation, where only 10 per cent of the available free energy is made use of. Nevertheless, once a process of this sort gets started, it is bound to spread. We know at the present day a number of bacteria which are called autotrophic, that is, which can nourish themselves entirely on inorganic ions, nitrate, phosphate, sulphydryl etc. These, however, cannot be representative of the primitive organisms, because they contain all the most complex enzymic systems.

It is certain that life in its first stages was entirely dependent on the pre-existence of organic molecules. But, correspondingly, as life spread, these organic molecules must have been consumed, and if no other process had supervened, early life must have burnt itself out. This, indeed, may have happened over and over again, because such burning, i.e. the decomposition of all complex molecules, must have restored the original state of affairs and started the whole process all over again, without leaving any notable traces. However, we know by our own existence that they did not all burn themselves out.

It seems most probable that the first crisis of primitive life was resolved by some organism chancing on another mode of collecting energy for chemical reactions, namely one which made use of the current input of solar light. At what stage this happened depends on whether the first organisms were adapted to work in the light or the dark. The latter seems more probable, especially as most of the reactions that we now know of in biochemistry are stopped by short-wave ultra-violet light. It is still, however, quite possible, either in the sea or, more probably, in a mud-bank, for the surface to be exposed to the full flux of sunlight, while the interior is carrying on with much reduced light intensity or in virtual darkness. Life creeping up towards the light might in this case find itself involved in the processes in which the absorption of medium ultra-violet or even visible light might assist the cyclic transformations and make them capable of utilizing less highly formed molecules. This is apparently what happens in the purple sulphur bacteria of today, which utilize simultaneously the energy of absorbed light and that derived from oxidations and reductions of iron and sulphur compounds.

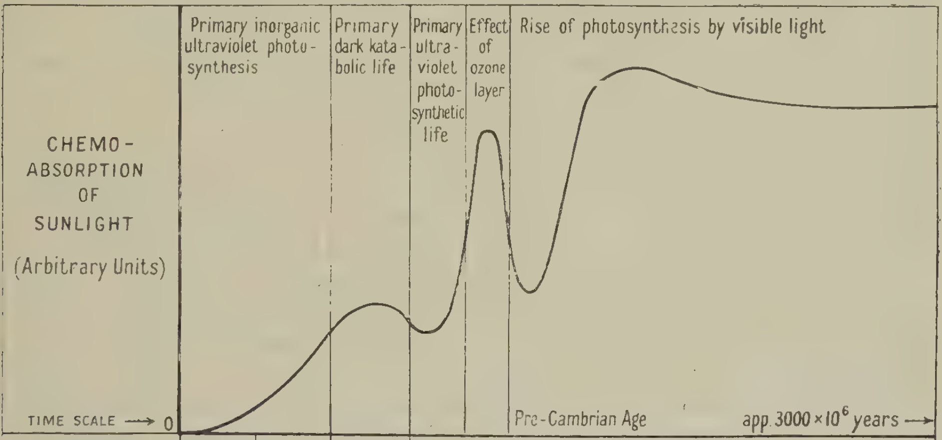
Effectively such a process is likely to occur the moment coloured substances capable of acting as sensitizers, as in photography, are synthesized. One group of these which seems to be especially favoured, possibly on account of their easy production, are the *porphyrins*, formed by condensing four pyridene derivatives around a metal ion. It is extremely suggestive that these porphyrins are found acting simultaneously as light receivers in the chlorophyll of all plants, and as respiratory pigments in cells, as oxygen carriers in many animals, and finally, and most strangely, associated with nitrogen absorption in the root nodules of peas. This points to all life being derived from a light-absorbing intermediate form—essentially a plant.

From the light-assisted metabolism of the purple bacteria to the full photosynthesis may not be a very wide step, but once the latter process is achieved, the future of life is assured. Modern research on synthesis shows that it is far from the simple reaction $CO_2 + H_2O = CH_2O + O_2$; that in fact the three processes of light absorption, carbon dioxide absorption and the liberation of free oxygen occur quite separately and may not all have been evolved at the same time. However, once photosynthesis started it made its mark on all subsequent living things, and was undoubtedly so successful that it wiped out earlier forms of life, except in very obscure holes and corners. One characteristic product of photosynthesis is sugar, and the universal prevalence of suger is only less marked than that of protein in the organic world, though all indications point to it as a much later derivative. Combined either with phosphorus or with purine bodies, or with both, sugars take part in an enormous number of enzyme systems, and particularly in nucleic acid, with its close associations with growth and reproduction.

The processes of respiration are so closely attached to those of photosynthesis that it seems probable that both occurred together in the first place, and only later became separated, when organisms developed predominantly one or the other process. Respiration, as already indicated, is a very much more efficient process than fermentation, and immediately increases the activity and range of living things. But full-scale respiration cannot have occurred until photosynthesis had been going on for some time and had produced the necessary oxygen. Once this happened there were two other direct consequences. In the first place, as already indicated, the oxygen drifting into the upper atmosphere interacted with the ultra-violet to produce an ozone layer which effectively blocked all the original syntheses on which life existed. But the very nature of photosynthesis prevented this having any serious effect on the production of organic from inorganic material. The danger may well have been in the other direction, a relatively unchecked building-up of carbon into cellulose and other sugar products. But the simultaneous existence of oxygen and combustible hydrocarbons put a premium on mechanisms of the reverse kind, and organisms specializing in respiration: bacteria, fungi and animals, tended to restore the balance. This is indicated in the diagram by a second maximum in the effective utilization of sunlight brought about by the reversal of the upsurge due to photosynthesis, by the increase of catabolic processes leading ultimately to the equilibrium in which we now live.

From the moment of the existence of an oxygen-rich atmosphere, the physical environmental conditions of life became essentially what they are now. This is as far as it is possible to go, at least at present, on the basis of purely physical and chemical knowledge, even assisted by what we know of biochemistry. For understanding development in the third act, that of organized life, we must rely on the ever deeper study of living organisms, both in structure and function, and use physical and chemical knowledge to interpret what is observed. A hundred years ago the discovery that the cell was the unit of life seemed to explain the common origin of the large observable organisms and tissues which were then studied in terms of some biological atom. In one sense this is still true. With the exception of the viruses, and possibly of some bacteria and the mycetozoa, all animals and plants are composed of cells; but the structure of the cell is likely to prove as complicated in terms of its constituents as that of the organism in terms of cells. The first fact which makes a science of cytology possible is that of cells reconstructed on the same basic pattern and developed, and particularly reproduced, by a set of complicated phenomena identical over the whole living kingdom.

The evolution of organisms must have been preceded by a period of the evolution of the cell. But the story of that evolution is certainly the most difficult part of the whole unravelling of biological processes, because it is at the same time the furthest removed from synthetic chemistry and analytical biology. Nevertheless in recent years, thanks to the new methods described above, much deeper understanding of the cell is becoming possible. The most striking general feature is the dual construction. The cell consists, inside a membranous envelope, of two parts, a nucleus itself provided with a membrane, and an external cytoplasm.



ATMOSPHERE	$CH_4.NH_3$ $OH_2.SH_2$	CO_2, N_2	N_2	N_2, O_2
HYDROSPHERE	Mainly inorganic ions – NH_4, CO_2	Mainly synthetic complex molecules	Living organism & diminishing amount of complex molecules	Living organism and inorganic ions — Na, K, Ca, Cl, SO_4, PO_4

This figure is entirely hypothetical except as to the sequence of the stages. The relative heights of the maxima may have been different and the stages may have overlapped much more than is shown, even to the extent of blotting out the maxima and minima.

Each of these volumes in turn contain other finer parts. The nucleus contains filamentous processes of varying number, the chromosomes, together with the associated nucleoli and centromeres, while the cytoplasm contains a world of particles of varying dimensions, particularly the mitochondria, the golgi bodies and a number of the smaller plastids, including the chloroplasts in the plants and the microplasts and other so-called organelles.

Many, if not all, these constituents appear to be self-reproducing. Therefore even in single-celled organisms there is considerable differentiation. Protoplasm is therefore far from a structureless substance, and to its microscopic complexity must correspond an even greater diversity of chemical and physico-chemical function, all pointing to an equally complicated history. I will not attempt to discuss the former of these, the cell equilibrium brought about through enzymes which seem to be attached to the mitochondria, and to the apparently dominating rôle of desoxyribose and ribose nucleic acids, the former being synthesized in the nucleus and its amount determining growth and division.

The cell, chemically speaking, may be considered as a little world, if not two little worlds, in which the whole set of chemical reactions characterizing some large volume of pre-organic life is concentrated. The essential feature of the chemical life of the cell is its extreme persistence along established lines of chemical reaction, and the way in which these characteristic reactions are reproduced over and over again in daughter cells. Schroedinger has pointed out that this exact reproduction is a quantum phenomenon in itself, and has attached the possibility of overcoming the entropy-increasing effect of normal diffusion by the quasi-solid (solid at least in one dimension) structures of the chromosomes themselves. Actually this uniformity of chemical action is by no means absolute, particularly in bacteria. The actual biochemistry of the cell can in fact be very violently varied by changes in the environment, and even the nucleus itself and the genetic processes are, as we now know, capable of random modification and of directed modification by specific chemicals. Their isolation in the past depended largely on the difficulty of getting the chemicals to the places where they could act.

Where physical methods can be of use is in interpreting the structures visible in the cell and in inferring others. Protoplasm is by no means a uniform substance. Electron microscope and X-ray researches show that it contains, quite apart from the relatively large structures previously referred to, others of intermediate size which, on account of their greater surface, are probably the seat of the greatest activities. The viscosity of the cell contents shows that it contains an appreciable amount of fibrous molecules. These are almost certainly of protein nature. One of the most interesting discoveries in this field is the extreme ability of the so-called globular, but more probably plate-like, proteins to aggregate in the form of fibres. This aggregation, if carried out without drastic conditions of temperature or acid, seems to be perfectly reversible. At least it has been observed to be so in the cases of insulin, actin, tropomyosin and fibrin. It would appear indeed that practically all fibres found in biological systems are made in this way from particulate proteins, for example paramyosin, where the particles can be seen in the electron microscope, collagen and keratin. Conversion of particulate to fibrous proteins is probably some form of chain-ring polymerization, but in the more labile forms this can only affect a small number of the available bonds. These fine fibres, running from 100 to 200 Ångströms in diameter, have of course very large surface areas and are capable, therefore, of carrying on almost as active chemical interchanges as free molecules. Indeed, it seems very probable that all the enzymes in cells are attached in this way, the most notable being the adenosin triphosphatase, which seems to be bound by myosin and to play an important part in muscle contraction.

An analogous phenomenon follows necessarily from the very existence of such fibrous molecules. Zocher and Bernstein (1929) first showed that in dilute clay suspensions the particles tend to arrange themselves in parallel sheets at regular intervals, which may be as great as 5000 A. I was able to study some phenomena in tobacco mosaic virus, as mentioned above, and in fact we can say now that all such fibrous molecules are attached to each other by long range forces, the range of which is dependent on the conditions of the medium, which may impose coagulation into microscopically visible fibres when the distances are small to a practically fluid and orientated gel when they are large.

The variability of the degree of fibrous nature of the cell contents is nowhere better shown than in the phenomenon of cell division, a complicated pattern which, however, is substantially the same for all cellular organisms. Some ten years ago I put forward a theory of mitosis based on the observation of these long range forces, and particularly on the analysis of the structure of the spindle-shaped tactoids (Bernal 1940). These were first observed by Zocher in iron oxide sols and later studied in more detail in tobacco mosaic virus. When a uniformly diffused area of rod-shaped particles is caused to aggregate, the forms taken by the aggregation are no longer spherical drops, but, on account of the long range forces, particles tend to remain in parallel orientation, giving rise to anisotropic surface forces and leading to an equilibrium in the form of a tactoid with a geometrical surface obeying the formula $\sigma_1 r_1 + \sigma_2 r_2 = \text{const.}$, where σ_1 and σ_2 are the principal surface tensions and r_1 and r_2 the principal radii of curvature. Exactly the same form is taken by inclusions of clear solution in a concentrated aggregate of long molecules. These, analogous to bubbles, are called negative tactoids. Such tactoids undoubtedly exist in dividing cells (see Bernal and Fankuchen 1941).

It would appear that the mechanism of their formation is somewhat as follows: at the instant of division, spontaneously or, perhaps, stimulated by changes going on in the nucleus, a small particle in the cytoplasm, the centrosome, causes a ring-chain polymerization of the proteins, proceeding as in insulin and forming a spherulite assembly of fibres radiating from a point. The centrosome divides and the spherulite correspondingly becomes a tactoid, or spindle. That this is the case has been shown very beautifully by the photographs of mitosis in polarized light. The existence of the tactoid produces an effective long range forces field. In such a field particles which do not mix with the system, such as the chromosomes, will tend to be driven to the equator where the particles are most parallel. This is the stage known as the metaphase.

The next stage marks the reversal of this process, but this appears to originate in a special section of each chromosome, the centromere. The centromeres of each paired chromosome, for the chromosomal division occurs at an earlier stage, proceed to separate, but in such a way as strongly suggests that each pair are the poles of a negative tactoid, and are in fact centres of disaggregation of the protein fibres. Thus they, so to speak, eat their way away from the equator towards the poles. That considerable forces are involved is shown by the fact that anomalous chromosomes which have not fully divided are literally torn in two. Much more research is needed to show whether this hypothesis is the correct one, but it has at least the merit of not invoking any forces other than those that can be demonstrated in other organic and even in inorganic systems such as ferric hydroxide and vanadium pentoxide gels.

Similar explanations based on the knowledge of long range forces may go far to explain the mechanics of chromosome pairing itself. In reproduction the chromosomes belonging to the father and mother nuclei are together in identical pairs. These must, in fact, seek each other out and arrange themselves mutually with extraordinary accuracy. The variation of nucleic acid material along the chromosomes, with its complicated pattern and the corresponding variation in long range forces, may serve to explain this process. Full interaction can only occur if there is a proper opposition of parts, and the difference between a good fit and a bad may insure that after a number of trials a fit is achieved in one place and then proceeds by zipper action over the rest of the chromosomes. We are only at the beginning of these studies, and far greater complexities will probably be revealed by further observation. But these complexities will act as a spur to further physical researches leading to their explanation.

So far I have spoken only of the interior of the cell. But for the survival of evolution of life, the relation of the cell to its environment seems now no such great mystery as it seemed to Henderson, considering, if our view is correct, that it was blocked out of that environment and represents simply a selective enclave of balanced chemical processes. The pre-condition of the existence of a cell is that it can maintain the concentration of its elements in a medium of very much lower concentration. This involves work, but it is a kind of work that the protein enzyme systems in the cells are particularly suited for, and for which the properties of lipoid membranes provide the necessary physical scaffolding.

In the further evolution of organisms we can distinguish size as the major conditioning factor. The uni-cellular organisms of a few microns in dimension must necessarily live in a world in which molecular forces rather than hydrodynamic ones are of prime importance. Small molecules on which nutrition and respiration depend must enter freely by diffusion. But diffusion is absolutely limited by size, and the larger an organism is, the smaller its effective surface, and the smaller the effective volume from which it can draw its nourishment. There are two ways out of this dilemma, and both have been followed by different organisms. The first is to grow only in one or two dimensions, to produce filamentous or leafy forms keeping a large surface. The second is motility, not the directed motility of the larger organisms, but simply moving for moving's sake, out of the comparative starvation of the immediate neighbourhood. The ceaseless motion of bacteria and protozoa had originally no other point, but once a controllable motile mechanism is evolved from such simple protein molecules as cilia, hardly more than a bundle of some eight or sixteen macro-molecules, future evolution towards directed motion becomes almost a necessity.

At this stage there are two directions open to organisms. One is to improve their biochemical mechanisms and be able to make use of a very dilute environment, and the other to move into a better environment ; that is the basic distinction between plants and animals. The first remain absorbers of individual molecules, and their motility is limited to extending their surfaces and arranging for the slow and steady flow of an internal environment. The motile forms, however, sooner or later are bound to bump into each other, and from that contact arises the possibility of digestion and, therefore, the possibility of acquiring new material in prefabricated pieces rather than molecularly. Motility in itself may fluctuate between total motility, in which the organism moves through the environment, and relative motility, in which the organism stays still and pushes the environment past it, as in the ciliates, sponges, oysters or barnacles. In the first case the essential new feature is direction, a movement along a chemical and thermal gradient. This involves the production of some kind of sensitive element at the head end, and some link between this and the motility. From these simple beginnings follows the evolution of the necessarily linked triad—sense organ, nerve system and effector.

The actual evolution of the higher organisms, using cell division to create a larger organism than is possible with single cells, may have been really only a mechanical necessity. Once the very small dimensions of the single cell are surpassed, forces utterly inappreciable on the micro-scale become important, such as turbulence of the medium and gravity. To resist these, greater strength than is provided by the protoplasmic framework is needed, and this is available by using the mechanical strength inherent in cell walls, either simply or strengthened by protein fibres or by cellulose. Where additional strength is required, mineral deposits like shell and bone can be added. Multi-cell organisms can afford to specialize, and our understanding of the physical nature of these specializations, such as the passing of nerve currents, the contraction of muscles or the secretion of chemicals, seem to show that they all use rather specially selected aspects of mechanisms common to all protein systems.

But now the particular function of the organ is no longer self-determining and absolute. It is part of a larger coordination. Modern mathematical analysis is showing us that the very fact of coordination, the very linkage mechanism itself, introduces new complexities and new possibilities of a very specific kind. Take, for example, one of the most complicated, the mechanism of vision. It is relatively straightforward to arrange a one-to-one connection between a single receptor in the retina and a single effector in the visual cortex. But this is only the beginning of the story. Such a system could see, but it could neither recognize nor follow. Three layers of cells are involved in grouping and sorting out the impulses of the cortex and creating the percepts of objects which are the basis of sensual experience of the higher animals. The arrangements of these layers of cells, as Weiner has shown, are essentially similar to the arrangements in modern electronic calculating machines with similar functions. An even higher complexity is required if we have to move from percept to concept, a process which in its fullness has only occurred in man, or possibly in some other social animals. Here the mutual relations of organisms originating in reproductive links have led to a still higher order of mutual interaction and corresponding permanency of cultural tradition.

This brief survey of the field of life does show that we are now beginning to see the possibility of accounting for it in terms that fit in with our intellectual and manipulative control of the non-living parts of environment. It is certain that this account will appear as absurd to those who come after us as did those of Borelli or of the early corpuscular mechanists of the seventeenth century. Yet even then this approach to rationality did make possible further advances of biology at that time, and so we may hope from the present analysis to achieve corresponding advances in biophysics. This does not mean any reduction of biology to simple physics. The complexity of the successive evolutionary levels mark definite orders of advance each with its own laws and each with its own internal unbalance leading forward to new levels. However, by elucidating structures, by following out mechanisms, we may at least clear the air of mystical interpretations which serve simply to conceal ignorance. Instead of concealing ignorance we may hope to reveal it, to find the places where the explanations will not fit, and sooner or later to find the means for studying just these points. In such a study we may reasonably expect to discover further and further points of ignorance and difficulty. But even if the task were endless it would still be worth while. Every new relation gained is an achievement of order, giving confidence and direction to future work, and at the same time providing for new controls over our biological environment, including our own bodies. The profundity of physics should not blind us to its relative simplicity in the scheme of human knowledge. Here in biophysics we have something so complex that it will require not only more industry but more intelligence than went into the earlier stages either of biology or of physics. Man has, in the past, used his knowledge to effect gross changes in his environment, first mechanical and then chemical. Only an understanding of the biological and social sciences can show how to make those changes of real benefit to man, and how to balance power with intelligence.

REFERENCES

Astbury, W. T., 1933, *Fundamentals of Fibre Structure* (Oxford : University Press).

Bernal, J. D., 1940, *The Cell and Protoplasm*, ed. F. R. Moulton, (Washington : Amer. Assoc. Adv. Sci.), p. 199.

Bernal, J. D., Bawden, F. C., Pirie, N. W., and Fankuchen, I., 1936, *Nature, Lond.*, **138,** 1051.

Bernal, J. D., and Fankuchen, I., 1941, *J. Gen. Physiol.*, **25,** 111.

Crowfoot, D., 1941, *Chem. Rev.*, **28,** 215.

Darwin, F., ed., 1887, *The Life and Letters of Charles Darwin*, vol. 3, p. 18.

Dauvillier, A., 1947, *Genèse nature et évolution des planètes* (Paris : Hermann), pp. 169 ff.

Engels, F., 1940, *Dialectics of Nature* (London : Lawrence and Wishart), p. 194 ff.

Goldschmidt, V. M., 1923–1937, *Skriften Norske Vidensk. Akad. K.*, **1.**

Haldane, J. B. S., 1928, *Rationalist Annual,* " The Origin of Life ", pp.148–153.

Hast, N., 1947, *Nature, Lond.*, **159,** 354.

Huxley, T. H., 1901, *Collected Essays,* " On the Physical Basis of Life ", vol. 1 (London : Macmillan).

Kuhn, W., 1946, *Experientia*, **2,** 10.

Lwoff, A., 1943, *L'Évolution Physiologique* (Paris : Hermann).

MacEwan, D. M. C., 1948, *Trans. Faraday Soc.*, **306,** 349.

Oparin, A. I., 1938, *The Origin of Life* (New York : Macmillan).

Perutz, M. F., 1949, *Proc. Roy. Soc.* A, **195,** 474.

Pirie, N. W., 1937, *Perspectives in Biochemistry*, eds. J. Needham, and D. R. Green, *The Meaninglessness of the Terms 'Life' and 'Living'* (Cambridge: University Press), pp. 21 ff.

Prigogine, I., 1947, *Étude thermodynamique des phénomènes irréversibles* (Paris : Dunod).

Turkevich, J., and Smith, R. K., 1946, *Nature, Lond.*, **157,** 874.

Verwey, E. J. W., and Overbeek, J. Th. G., 1948, *Theory of the Stability of Lyophobic Colloids* (Leyden : Elsevier).

Zocher, H., and Birnstein, V., 1929, *Z. Phys. Chem.* A, **142,** 113.

A Stereographic Projector

By A. E. De BARR
G.K.N. Research Laboratories, Wolverhampton

MS. received 26*th April* 1949

ABSTRACT. An optical device is described by which the effect of rotation on the stereographic projection of a crystal can be observed directly. The application of the device to the construction of pole figures from x-ray data is described.

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§1. INTRODUCTION

Many crystallographic problems are most readily discussed and solved by the use of the stereographic projection. Measurements of the angular relationships in and between crystals and calculations of the angular relations involved in tilting or cutting crystals are readily carried out in this way. The most general type of problem involves the determination or the description of the movement of a plane or set of planes when the crystal is moved in a prescribed manner. With a stereographic projection and stereographic and polar nets rotations of any pole about axes normal to or in the plane of the projection may easily be made. Rotations about other axes are not so simple, but can always be resolved into components about axes normal to or in the plane of the projection (De Barr 1948). However, when several rotations are involved and it is required to follow the movements of many poles, the graphical work involved becomes laborious. The principle to be described in this note enables the effect of any rotation on any number of poles to be observed immediately.

§2. PRINCIPLE

Referring to Figure 1, P is the pole to be projected, P′ is its stereographic projection and O is the centre of the reference sphere. X′OX is the plane of projection. If now the normal to the plane to be projected is represented by a small pencil of light directed along OP and a screen is placed surrounding O and so

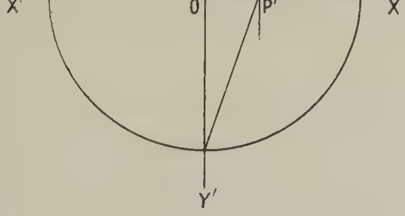

Figure 1.

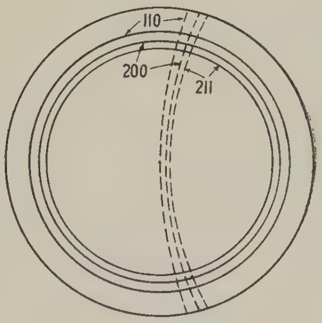

Figure 2. Reflection circles for iron using MoKα radiation
———— X-ray beam normal to the plane of projection.
- - - - X-ray beam at 10° to the plane of projection.

situated that the pencil of light intersects the screen at R where R lies on the perpendicular to the projection plane through P, then, viewed normal to the projection plane, the spot of light on the screen appears in the position of the stereographic projection of P. The screen is a surface formed by revolution about YOY′ of the locus of all such points as R. It may easily be shown that, referred to axes OXY as shown, the equation to this locus is $x^2=r^2-2ry$, where r is the radius of the projection sphere. The form required for the screen is therefore that of a paraboloid of revolution.

If now a screen of this form is correctly placed over a projection system such that the normals to the planes to be projected are represented by pencils of light, then each such pencil will produce on the screen a spot which, viewed normally to the plane of projection, corresponds with the stereographic projections of the planes. The projection system may be rotated in any manner and, provided that the pencils of light always proceed from the centre of the reference sphere, the spots of light on the screen represent the stereographic projections of the poles, if viewed normally.

§3. APPARATUS

Figure 3 shows a convenient arrangement for a stereographic projector using the principle outlined above. The projection system consists of a small source of light A mounted at the centre of an opaque, hollow hemisphere B which is pierced with small holes representing the poles to be projected. (It is convenient to drill in one hemisphere all the holes likely to be required and to fill in with wax those not needed at any one time.) This system is supported with the light source at the centre of a large, hollow hemisphere C resting on ball bearings in the frame D in the manner of a universal joint. The screen E is supported from the frame so that its centre is vertically above the light source by an amount $r/2$.

A suitable parabolic screen can be made from $\frac{1}{16}$ in. Perspex, softened in hot water and pressed to shape in a wooden mould. The two portions of the mould can be made on a lathe from a template constructed to the required screen profile. Allowance must be made for the thickness of the Perspex when making the mould. The screen can be given a ground surface with grinding paste.

§4. APPLICATION TO THE CONSTRUCTION OF POLE FIGURES

In the construction of pole figures from X-ray diffraction photographs, the number of photographs required can be considerably reduced if all the diffraction data recorded on the photograph are used (Wood 1948). Thus diffraction photographs of iron with Mo Kα radiation show diffraction arcs from 110, 200 and 211 reflections and all this information can be used to construct, say, a 100-pole figure. To use it it is necessary to know, e.g., the positions of the 100 poles when a particular 110 or 211 pole is in a reflecting position. This information is readily obtained from the stereographic projector if a plane transparent screen F carrying the reflection circles (Barrett 1943) for all the planes involved is placed over the projector as in Figure 3. Such a screen is shown in Figure 2. The projector is rotated until, say, a 110 pole lies on the 110 reflection circle in a position corresponding to that of a 110 diffraction arc on the X-ray photograph. The corresponding positions of the 100 poles can then be observed directly on the screen and plotted on the pole figure. The actual rotation of the projector as described above must be adjusted until the results from all the poles are consistent.

If the small light bulb is replaced by a Pointolite source and the lower surface of the screen is coated with fluorescent paint (luminescent powder E(P), supplied by G.E.C. Ltd., has been found suitable for use with a 30 c.p. Pointolite bulb), actual pole figures can be depicted on the screen by moving the projection system around as desired for several seconds. On switching off the lamp, the luminescent pattern remaining represents the pole figure. A model of the stereographic projection showing this feature was exhibited at the Physical Society Exhibition in 1948.

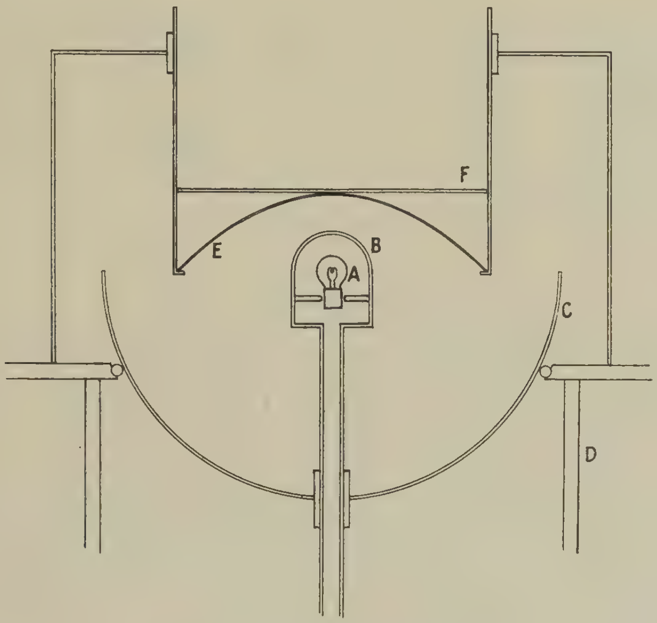

Figure 3. A stereographic projector.

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REFERENCES

Barrett, C. S., 1943, *Structure of Metals* (New York : McGraw-Hill).
De Barr, A. E., 1948, *J. Sci. Instrum.*, **25**, 102.
Wood, J. K., 1948, *J. Appl. Phys.*, **19**, 784.

A Study of Magnetic Viscosity

By R. STREET and J. C. WOOLLEY
The University, Nottingham

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ABSTRACT. A general analysis of magnetic viscosity based on activation energy concepts is given. A particular case is considered of the application of the theory to the phenomenon occurring in materials in which bulk magnetization proceeds by the rotational movement of the vectors of single domains, which process, according to a recent theory of Stoner and Wohlfarth, may occur in certain high coercivity alloys. Experiments have been made, using a magnetometer method, with specimens of alnico maintained at various temperatures within the range −187° c. to 250° c. The results obtained are found to be in good agreement with the theoretical analysis.

§1. INTRODUCTION

If the magnitude of an external magnetic field applied to a ferromagnetic specimen is suddenly increased then, in general, the consequent change of the intensity of magnetization is not correspondingly rapid. For most ferromagnetic substances there is a time lag between the magnetic field and the magnetization which may be accounted for almost completely in terms of the production of eddy currents within the specimen. However, with certain materials the persistence and magnitude of the change of magnetization are much too great to be accounted for by eddy current formation and this phenomenon is now generally called "magnetic viscosity". Previous investigations of the phenomenon have been summarized by Becker and Döring (1939) and the following qualitative observations have been made. The effect (i) is most noticeable on the steepest part of the magnetization curve, (ii) is temperature sensitive, (iii) as far as soft iron specimens are concerned, is influenced by the presence of small quantites of impurities such as carbon and nitrogen, and (iv) closely resembles analogous mechanical and dielectric phenomena.

Becker and Döring have pointed out that no comprehensive theory of magnetic viscosity exists. They have therefore developed the analogy between dielectric and magnetic "after effect"; this involves assigning relaxation times to the ferromagnetic domain processes which give rise to magnetic viscosity. Under certain conditions, they predict that the change in the intensity of magnetization is a logarithmic function of time. Again, Snoek (1938) has developed a theory which accounts successfully for the phenomenon known as the time decrease of permeability which he has shown to be closely connected with magnetic viscosity; owing to certain limitations in this theory it is, however, not possible to compare its predictions with experimental observations on magnetic viscosity.

In the theory given below the problem is approached by considering that magnetic viscosity is an activation-energy process. Physical mechanisms which involve such activation energies can be visualized, and it is shown that, in at least one case, namely that of a high coercivity alloy, the required mechanism is already implicit in a recent theory of heterogeneous alloys (Stoner and Wohlfarth 1948.)

§2. THEORY

The analysis given in this section is essentially formal and follows that already given by Smith (1948) in his theory of transient creep in metals; consideration of the physical principles underlying the assumptions made here will be deferred until §6.

It is first supposed that at some stage during the magnetization of a ferromagnetic substance, following upon a sudden increase in the applied magnetic field, a certain number of the domains have their magnetization vectors in positions which may be described as "metastable". Thus, a small increase in domain energy makes little difference to the vector orientation, but a larger energy change initiates a transition to stable equilibrium which involves a discontinuous change of orientation. The mechanism envisaged is therefore, in essentials, an activation process.

At a time t after the sudden increase in applied magnetic field let the number N of domains characterized by activation energies lying between E and $E+dE$ be $f(E)\,dE$. Then the time rate of change of N due to thermal activation with the specimen maintained at constant absolute temperature T will be given by

$$\frac{dN}{dt} = -Cf(E)\,dE \exp(-E/\boldsymbol{k}T) \qquad \ldots\ldots(1)$$

assuming that successful activations do not produce additional metastable states with activation energies in the range from E to $E+dE$. Equation (1) is satisfied by

$$f = f_0 \exp(-\lambda t) \quad \text{where} \quad \lambda = C\exp(-E/\boldsymbol{k}T). \qquad \ldots\ldots(2)$$

C is a constant and f_0 denotes the value of the function f at time $t=0$.

If each activation contributes an average amount $\bar{i}$ to the resolved part of the magnetization along the direction of the applied field, then the activation of dN domains gives a mean increase of the intensity of magnetization of the specimen of

$$\bar{i}Cf_0 \exp(-\lambda t) \exp(-E/\boldsymbol{k}T)\,dE\,dt.$$

Taking into account all values of activation energy, the time rate of increase of the intensity of magnetization is

$$\frac{dI}{dt} = \bar{i}C\int_{E_{\min}}^{E_{\max}} f_0 \exp(-\lambda t) \exp(-E/\boldsymbol{k}T)\,dE \qquad \ldots\ldots(3)$$

and this integral may be evaluated only if the limits and the form of the function f_0 are known. Consequently three simple cases which are relevant to the present problem will be considered below.

(i) Let us assume that f_0 is independent of E and has a constant value p. (This is the case considered by Smith (1948).) Then

$$\frac{dI}{dt} = \bar{i}Cp\int_0^{\infty} \exp(-\lambda t) \exp(-E/\boldsymbol{k}T)\,dE, \qquad \ldots\ldots(4)$$

taking all values of activation energy between 0 and ∞, as a first approximation. From equation (2)

$$d\lambda = -(C/\boldsymbol{k}T)\exp(-E/\boldsymbol{k}T)\,dE$$

and substituting in equation (4)

$$\frac{dI}{dt} = \bar{i}p\boldsymbol{k}T\left[\frac{1}{t}\exp(-\lambda t)\right]_{\lambda=C}^{\lambda=0} \qquad \ldots\ldots(5)$$

$$\frac{dI}{dt}=\frac{\bar{i}p\boldsymbol{k}T}{t}\{1-\exp(-Ct)\}.$$

If $Ct \ll 1$

$$\Delta I=\bar{i}p\boldsymbol{k}T(Ct)+\text{const.} \qquad \ldots\ldots(6)$$

and if $Ct \gg 1$

$$\Delta I=\bar{i}p\boldsymbol{k}T\log t+\text{const.} \qquad \ldots\ldots(7)$$

In these expressions the symbol ΔI denotes the contribution to the total intensity of magnetization by the thermal activation of the domains, measured at a time t after the increase in applied field.

Which of the assumptions made in equations (6) and (7) is more nearly correct in any given case may best be decided by experiment. In the experimental work described below it is shown that the observed variation of ΔI is a linear function of $\log t$ as predicted in equation (7). Consequently, in the other theoretical considerations in this section it will be assumed that $Ct \gg 1$ for the values of time involved in the experimental studies.

(ii) Equation (7) predicts that the intensity of magnetization of the specimen increases indefinitely (proportional to $\log t$) although in practice the magnetization must eventually reach some finite value. This difficulty is a necessary consequence of the assumption that $f_0=p$ for all values of activation energy from 0 to ∞; this also implies that, if $p \neq 0$, an infinite number of domains may be activated. Physical considerations suggest that a better approximation for the distribution function f_0 is

$$\left.\begin{aligned} f_0(E)&=p \quad \text{for} \quad 0<E<E_0 \\ f_0(E)&=0 \quad \text{for} \quad E>E_0 \end{aligned}\right\} \qquad \ldots\ldots(8)$$

where E_0 is some limiting upper value of the activation energy. The total number of domains which may be activated, N_0, is, from equation (8), pE_0.

Equation (5) now becomes

$$\begin{aligned}\frac{dI}{dt}&=\bar{i}p\boldsymbol{k}T\left[\frac{1}{t}\exp(-\lambda t)\right]_{\lambda=C}^{\lambda=\lambda_0} \\ &=\frac{\bar{i}p\boldsymbol{k}T}{t}\{\exp(-\lambda_0 t)-\exp(-Ct)\}, \qquad \ldots\ldots(9)\end{aligned}$$

where $\lambda_0=C\exp(-E_0/\boldsymbol{k}T)$.

The experimental work referred to in (i) above shows that in practice equation (7) represents the observed behaviour with some accuracy and thus, in any modifications such as those of equation (8), the upper limit of the integration must be of the form $\lambda \to 0$, and this is so if $E_0 \gg \boldsymbol{k}T$. Hence equation (9) may be reduced to

$$\Delta I=\bar{i}p\boldsymbol{k}T\int_{t_C}^{t}\frac{1}{t}\exp(-\lambda_0 t)\,dt+I_C, \qquad \ldots\ldots(10)$$

where I_C is the value of ΔI at a time t_C after the change in the applied magnetic field when the term $\exp(-Ct)$ becomes negligible with respect to $\exp(-\lambda_0 t)$; and $t_C \ll 1$ since E_0 is assumed $\gg \boldsymbol{k}T$. For the present purposes only an indication of the function (I, t) is required and this is given as follows.

Expanding

$$\begin{aligned} t^{-1}\exp(-\lambda_0 t)&=t^{-1}-\lambda_0+\lambda_0^2 t+\ldots. \\ &\simeq t^{-1}-\lambda_0, \text{ if } t \ll 1/\lambda_0, \end{aligned}$$

then

$$\Delta I \simeq \tau p\boldsymbol{k}T\{(\log t)-\lambda_0 t+\text{const.}\}. \qquad \ldots\ldots(11)$$

Equation (11) shows that to the order of approximation assumed, i.e. $t \ll 1/\lambda_0$, the value of ΔI is reduced from the value predicted by the simple $\log t$ function by an amount $\lambda_0 t$. In the experiments described below this correction term became significant only for large values of t, and, in the period of time usually considered here, was negligible.

The final steady value attained by ΔI is found by taking the upper limit of the integral of equation (10) equal to ∞. Standard tables of this integral, $-\mathrm{Ei}(-t_C)$, (Jahnke and Emde 1938), show that it is finite for integration between the required limits, t_C and ∞. Consequently, the distribution function assumed, equation (8), leads to a final constant value of ΔI at $t=\infty$.

(iii) A particularly simple form of distribution function is obtained by considering that all the domains capable of activation after a sudden increase in the applied field are exactly similar and are characterized by the same activation energy E_s. Then

$$dI/dt = \bar{i} N_0 \lambda_s \exp(-\lambda_s t)$$

where

$$\lambda_s = C \exp(-E_s/\boldsymbol{k}T)$$

and

$$\Delta I = -\bar{i} N_0 \{\exp(-\lambda_s t) + \mathrm{const.}\}. \qquad \ldots\ldots(12)$$

This result is formally equivalent to that obtained by Snoek (1938).

The experimental work described below has been directed towards the determination of the form of the $(\Delta I, t)$ function and also to the quantitative examination of the influence of temperature on magnetic viscosity.

§3. SPECIMENS

The specimens used in the investigations described here were rods of alnico having the following approximate percentage composition: Al 10, Ni 18, Co 12, Cu 6, Fe 54, and dimensions 30 cm. long and 0·65 cm. diameter. Two such rods were available, one as cast or untreated, and the other heat-treated by maintaining it at a temperature of 1250° c. for 20 minutes, allowing it to cool at a rate of about 2° c. per second and then annealing it at 600° c. for about 2 hours. These rods were chosen for the experimental work since, of the various high coercivity materials available, it was known from a previous investigation (Street and Woolley 1949) that the phenomenon of magnetic viscosity was considerably more marked in these specimens than in soft iron. The results given below were all obtained with the untreated specimen, but the behaviour observed with the heat-treated specimen was essentially similar.

§4. METHOD AND APPARATUS

The specimens were mounted in either an electrically heated tubular oven (the heater of which was wound non-inductively and fed with D.C.) when temperatures up to 250° c. were required, or a Dewar vessel containing either liquid air or "dry ice" moistened with alcohol as required. External magnetic fields were applied by means of a surrounding water-cooled solenoid of length 60 cm. capable of producing a maximum field intensity of about 800 oersted. The required stability of the magnetic field could only be obtained by supplying the current to the solenoid from a 280 v. battery of high capacity accumulators.

Measurements of the intensity of magnetization of the specimens were made magnetometrically, the magnetometer being set in the "broadside-on" position with respect to the solenoid. The magnetometer consisted of a small bar magnet of approximate dimensions $1 \times 0{\cdot}1 \times 0{\cdot}1$ cm. and a small concave mirror waxed to a damping vane of mica supported by a single phosphor bronze strip ($21{\cdot}5 \times 0{\cdot}012 \times 0{\cdot}0012$ cm.) soldered at its upper end to a tensioning spring and at the lower end to a torsion head. The usual coils to neutralize the magnetic field produced by the solenoid alone at the magnetometer needle and to calibrate the instrument were incorporated in the apparatus. In addition, the sensitivity of the magnetometer could be decreased by a large factor by passing a current through another subsidiary coil which augmented the horizontal component of the earth's magnetic field at the magnetometer needle. This coil was only energized when large variations of the intensity of magnetization were produced, e.g. when the specimen was being brought into a cyclic condition; the actual measurements were made with no current flowing in this coil. It was checked that under normal working conditions the deflection of the magnetometer needle was a linear function of the deflecting field. The change in magnetometer deflection was 1·00 cm. when the deflecting field changed by 0·00107 oersted and this field change was produced by a variation of 3·45 gauss in the intensity of magnetization of the specimen.

Temperature measurements during the magnetic experiments were made by a calibrated thermocouple having one junction attached to the mid-point of the specimen. It was shown by means of this thermocouple that the temperature of the specimens could be maintained almost indefinitely at any constant temperature between 15° c. and 250° c. to within about 2° c.; with solid carbon dioxide in the Dewar vessel the temperature was -78 ± 1° c. for approximately 40 minutes, and when liquid air was used the temperature was maintained at -187 ± 1° c. for 30 minutes.

When measurements of magnetic viscosity were made at a point on the hysteresis cycle of a specimen corresponding to a final steady applied field H the following procedure was adopted. The specimen was brought into a cyclic state and then the whole apparatus was allowed to reach thermal equilibrium with an applied field H. If observations were to be made with H not equal to the coercive field, ${}_IH_c$, i.e. the intensity of magnetization of the specimen was not zero, then the deflection of the magnetometer was made equal to zero by passing the appropriate current through the calibration coil of the instrument. The specimen was taken through an almost complete hysteresis cycle so that the applied field now was less than the original value, H, by an amount ΔH. After an interval of time the field was suddenly increased by ΔH and observations were made of the magnetometer deflection, and hence the change of intensity of magnetization of the specimen, as a function of the time which had elapsed since the sudden increase in applied field. During the measurements the solenoid current varied by not more than $\frac{1}{4}$% and no changes in the calibrating coil current, when used, could be detected.

§5. RESULTS

A typical set of measurements is shown in Figure 1 in which ΔI is plotted directly against t; these measurements were made with the alnico rod maintained at room temperature and the final value of the applied field was approximately the coercive field. This figure also includes two curves which have been calculated from equations (7) and (12) respectively and fitted, as far as possible, to the experimental points. Over the time range involved it is obvious that equation (7) gives the better fit. Also the experimental results appear to lie below the curve of equation (7) at larger values of t and thus some degree of support for the modified equation (11) is forthcoming. The usual way of presenting these results was in the form of curves of ($\Delta I, \log t$) and examples of these are shown in Figure 3; the slopes of such lines will be denoted by S.

Before it was possible to investigate the variation of S as a function of the absolute temperature of the specimen it was necessary to determine how S was affected by variation of the experimental conditions. This preliminary work was carried out at a constant temperature of 15° c., and the final magnetic field after the discontinuous change was always the coercive field. S was found to be independent (i) of the magnitude of the discontinuous field change provided that this was greater than 10 oersteds, and (ii) of the time the specimen was maintained under a steady applied field immediately before the measurements were begun if this time was greater than 2 minutes. The behaviour observed in (i) is similar to that of Heaps (1938); result (ii) may be compared with the analysis of Ewing's results for a soft iron specimen (1889), for if these are plotted as ΔI against $\log t$ they show that the S value found when the waiting period was 3 minutes is greater than its value for a waiting period of 60 minutes. Actually, Ewing's work was confined to the examination of magnetic viscosity over the virgin curve of magnetization of the specimen.

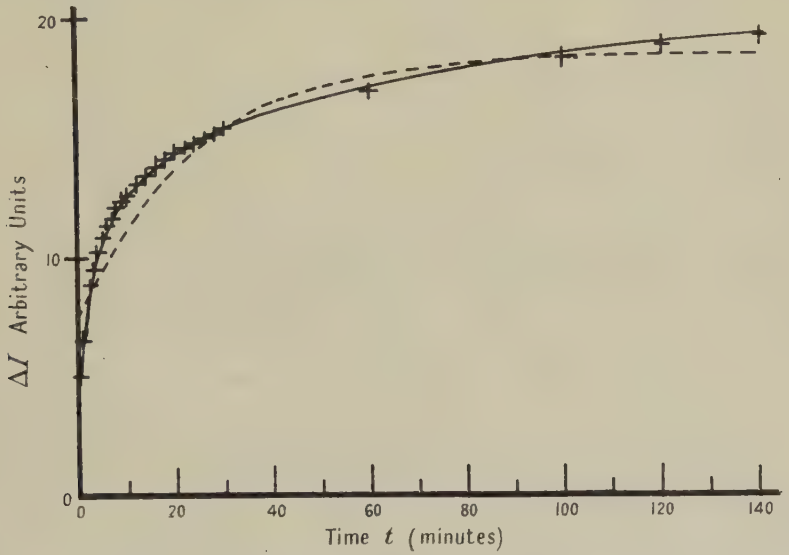


Figure 1. Variation of magnetization as a function of the time after a sudden increase in the applied magnetic field.

——— Theoretical curve calculated from $\Delta I = S \log t + \text{const.}$

– – – – – Theoretical curve calculated from $\Delta I = I_0 + I'\{1 - \exp(-t/\tau)\}$.

+ + + Experimental points for untreated alnico specimen at 15° c. with coercive field applied.

Note. The zero of the ordinate scale is arbitrary.

The magnitude of S was found to be a function of the intensity of magnetization of the specimen at which the observations were made and, as far as the hysteresis cycle was concerned, no appreciable changes of the magnetization were observed in steady applied fields except near the coercive field point. The variation of S as function of the magnetization I and the final steady field H is shown in

Figures 2 (*a*) and (*b*) respectively. (In these measurements the discontinuous changes in applied magnetic field were of such a magnitude that the intensity of magnetization changed by approximately 200 gauss in each case, and the waiting period, described in the previous paragraph, was about 5 minutes.) The slope of the hysteresis loop dI/dH is also plotted on these figures as a function of I and H, and it will be seen that the maximum value of S occurred when dI/dH had the greatest value. In similar measurements carried out over the virgin curve of magnetization a smaller maximum value of S was observed and again this maximum value was found in the region of maximum dI/dH.

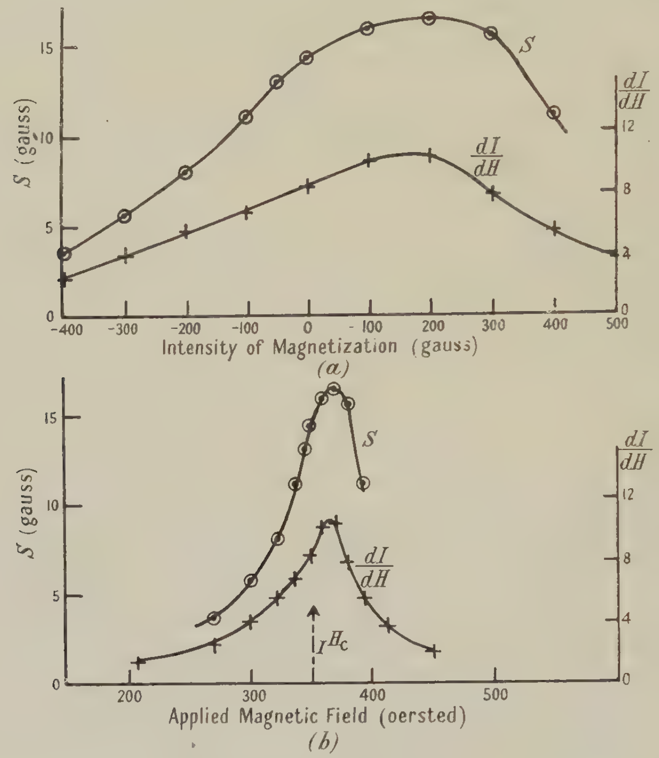


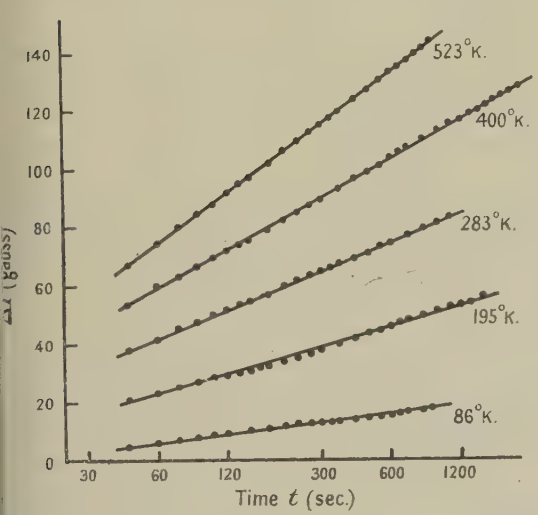
Figure 2. Variation of S and dI/dH as a function of both the intensity of magnetization and the applied magnetic field intensity.

The experiments to determine the temperature dependence of S were simplified by having the final applied field equal to the coercive field; the discontinuous change in intensity was approximately 200 gauss after a waiting period of 5 minutes. The results obtained at different temperatures between $-187°$ c. and 250° c. are plotted in Figure 3 in the form of curves of $(\Delta I, \log t)$ and Figure 4 is a plot of S as a function of the absolute temperature T. The straight line of Figure 4 shows that S was a linear function of the absolute temperature and this indicates that $\bar{i}p$ (equation (7)) was independent of temperature and had a value of $3{\cdot}6 \times 10^{14}$ gauss per erg.

In addition to the magnetometer method described above, other ways of demonstrating magnetic viscosity in these alnico specimens have been used and these will now be described briefly.

(i) The amplitude of magnetostrictive oscillations set up in a ferromagnetic specimen is critically dependent on the magnetization near the coercive field. Any changes in magnetization due to magnetic viscosity may thus be detected by variation of the amplitude of the oscillation and this phenomenon has already been described (Street and Woolley 1948).

(ii) The specimen was placed inside a search coil of 8,000 turns wound in the form of a solenoid. This search coil was connected to a sensitive galvanometer after a discontinuous change in the field applied to the specimen had taken place, when the rate of change of magnetization due to magnetic viscosity induced in the search coil an E.M.F. which gave a detectable galvanometer deflection. The experimental results showed that the galvanometer current was inversely proportional to the time t after the change in field, over the time period during which measurable deflections were obtained, about 300 seconds. Simple analysis of the experimental arrangement showed that this result would be expected if $\Delta I = S \log t$ as indicated by equation (7). The experiment was carried out at room temperature and the value of S was found to be 15·1 gauss, which compared satisfactorily with the corresponding value of 14·3 gauss obtained by the magnetometric method.



igure 3. Variation of magnetization as a logarithmic function of time t after a sudden increase in applied magnetic field. The temperatures at which the experiments were made are indicated on the curves.
Note : The zero of the scale of ordinates is arbitrary and the relative positions of the graphs have been chosen for convenience in drawing.

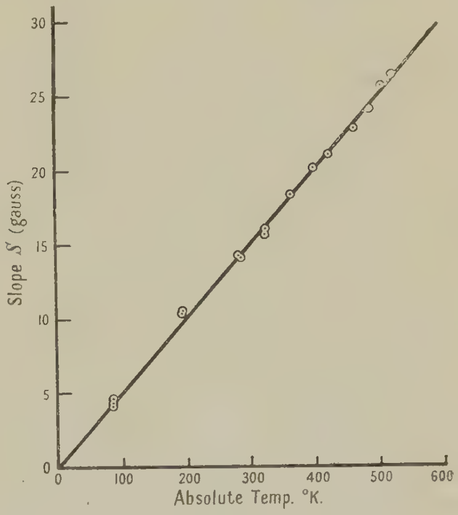


Figure 4. Variation of S, the slope of (ΔI, log t) curves, as a function of absolute temperature.

(iii) If the intensity of magnetization of a specimen lags behind the applied field due to magnetic viscosity then it would be expected that different magnetization curves would be obtained by using first a magnetometer method, during which the magnetization was allowed to approach its steady value, and secondly, a ballistic method. Figures 5 (*a*) and (*b*) respectively show two sets of magnetization curves obtained by magnetometric and ballistic methods; in the former case measurements of magnetization were made at times up to 30 minutes after the field change to allow for magnetic viscosity effects. In Figure 5 (*a*) the virgin curve lies almost completely within the hysteresis loop whereas in Figure 5 (*b*) the virgin curve lies significantly above the loop; moreover, the maximum intensity of magnetization is greater in (*a*) than in (*b*). The reasons for these differences are that appreciable magnetic viscosity occurs over some regions of the hysteresis cycle and there is a smaller viscosity effect for the virgin curve. The area of the hysteresis loop in (*a*) is larger than that shown in (*b*) by about 7%.

(iv) The alnico specimen was mounted in an apparatus for the examination of magnetostriction and it was shown that near the coercive point the magnetostrictive change in the length of the rod, produced by a sudden increase in the applied field, was not instantaneous but that an appreciable interval of time elapsed before a final value was reached. The coefficient of magnetostriction of the alnico for a maximum field of 850 oersteds was $+9{\cdot}0\times10^{-6}$ which, with the experimental arrangement adopted, was represented by a scale deflection of 23 mm. Consequently, it was not possible to make accurate measurements of the viscosity effect, but it would appear that such measurements could be made with apparatus of greater sensitivity.

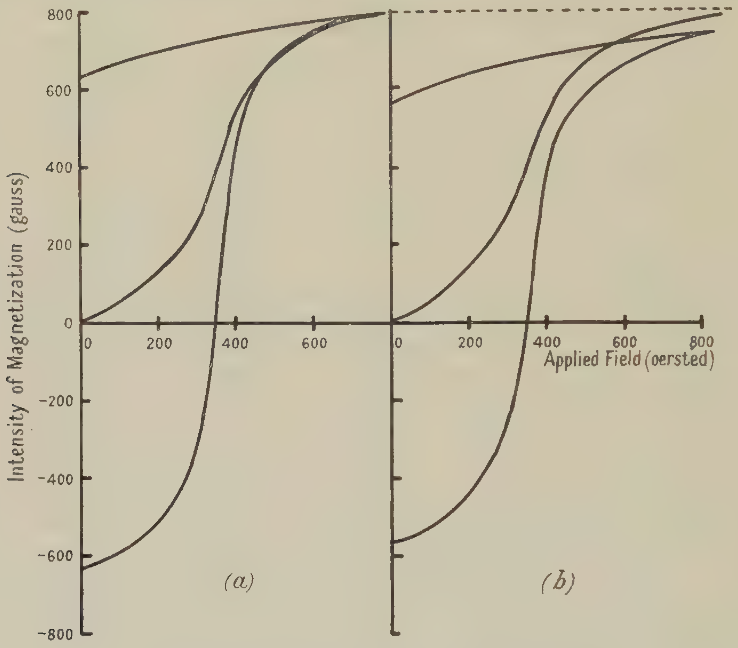


Figure 5. Magnetization curves for alnico.
(*a*) Obtained magnetometrically.
(*b*) Obtained ballistically.

§6. DISCUSSION

By assuming that over certain ranges of magnetization the directions of the magnetization vectors of ferromagnetic domains may be influenced by thermal activation, it is possible to formulate a theory of magnetic viscosity which is consistent with experimental observation. The relation between ΔI and t given in equation (7) has been shown to represent the experimental results obtained with the alnico specimens used here, and it also adequately describes, within limits, other results for soft iron specimens described by Ewing (1889) and by Richter (1937). Certain assumptions have been made in the derivation of equation (7) and the justification of these assumptions must be implicit in any proposed physical mechanism of the phenomenon. Snoek (1939) has already shown that magnetic viscosity cannot be detected in extremely pure soft iron, but as little as 0·01% of carbon and nitrogen impurity does produce pronounced magnetic viscosity. If, as analysis of the results appears to indicate, the activation energy hypothesis given above applies in the case of soft iron, the metastable states involved must be associated with the presence of small amounts of carbon etc. In Snoek's hypothesis it is assumed that magnetic viscosity is due to the diffusion of the impurity atoms into the Bloch zones or boundaries between domains. In view of the description of diffusion in solids given by Glasstone *et al.* (1941), it seems probable that such diffusion of impurity atoms is an activation energy process. A similar diffusion mechanism might account for magnetic viscosity in the alnico specimens. However, Richter's measurements seem to indicate that the variation of S (the slope of the $(\Delta I, \log t)$ curve) with temperature in the case of soft iron is essentially different from that shown in Figure 4. In the latter case S is a linear function of absolute temperature whereas for soft iron S appears to be substantially independent of temperature. This indicates that Snoek's explanation may not be appropriate in the case of alnico and that here a different mechanism may be involved. Such a mechanism is suggested by the theoretical treatment of heterogeneous alloys given by Stoner and Wohlfarth (1948).

In their theory it is suggested that in high coercivity alloys there are particles, the dimensions of which are smaller than the critical values necessary for the formation of ferromagnetic domain boundaries; these particles are embedded in a less ferromagnetic matrix. Magnetization processes, as far as these single domains are concerned, are therefore confined entirely to rotation of the magnetization vectors, and it has been shown that over certain regions of the hysteresis loop the directions of the domain vectors suffer discontinuous changes. In the theoretical case considered by Stoner which is most nearly appropriate to the present problem of a polycrystalline alnico specimen, these discontinuous changes of orientation occur over a range of the hysteresis loop near the coercive force point. The free energy of a domain has a minimum value both before and after a discontinuous change of orientation, and transition from the initial to the final state may only occur if sufficient energy to overcome the intervening potential barrier is supplied. This transition may therefore be achieved either by an increase in the applied magnetic field or by a change in the strain energy of the domain, and thus stress fluctuation due to thermal agitation may provide the activation energy required. Thus Stoner's analysis accounts naturally for the potential barriers which may be overcome by thermal activation energy. In addition, for any single domain, only one discontinuous change in orientation is possible on the descending branch of the magnetization curve, i.e. a discontinuous change in orientation does not produce domain states which may undergo further activation; this is an assumption which was made in the derivation of equation (7). Evidence for the similarity between the effects of applied magnetic field and stress due to thermal agitation is given in Figure 2 (*a*) of (S, I) and $(dI/dH, I)$. Within the limits of experimental error the maxima of the curves occur at the same value of I.

The variation of S as a function of absolute temperature (Figure 4) shows that under the conditions described (i.e. measurements at the coercive force point in the temperature range from 86° K. to 523° K.) the product $\bar{ip}$ is independent of temperature. This result might be expected from Stoner's theory of single domains for, at temperatures well below the Curie point (i.e. where the specific saturation intensity of the domains is constant) the theory does not involve temperature. Also at these temperatures there is no possibility of large scale migration of atoms, and thus the size of the ferromagnetic particles is independent of temperature.

Hence the theory of Stoner and Wohlfarth appears to justify the assumptions made in the analysis given in §2 above. Since the experimental results are in good agreement with this analysis, the work described here provides some experimental evidence for the validity of the single domain concept.

ACKNOWLEDGMENTS

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REFERENCES

Becker, R., and Döring, W., 1939, *Ferromagnetismus* (Berlin : Springer).
Ewing, J. A., 1889, *Proc. Roy. Soc.* A, **46**, 269.
Glasstone, S., Laidler, K. J., and Eyring, H., 1941, *Theory of Rate Processes* (New York : McGraw-Hill).
Heaps, C. W., 1938, *Phys. Rev.*, **54**, 288.
Jahnke, E., and Emde, F., 1938, *Funktionentafeln*, 3rd Edition (Berlin).
Richter, G., 1937, *Ann. Phys., Lpz.*, **29**, 605.
Smith, C. L., 1948, *Proc. Phys. Soc.*, **61**, 201.
Snoek, J. L., 1938, *Physica*, **5**, 663 ; 1939, *Ibid.*, **6**, 161.
Stoner, E. C., and Wohlfarth, E. P., 1948, *Phil. Trans. Roy. Soc.*, **240**, 599.
Street, R. and Woolley, J. C., 1948, *Proc. Phys. Soc.*, **61**, 391 ; 1949, *Proc. Phys. Soc.* B, **62**, 141.

The Decay of ^{170}Tm and ^{186}Re

By P. J. GRANT and R. RICHMOND
Cavendish Laboratory, Cambridge

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ABSTRACT. The radiations from the active isotopes ^{170}Tm and ^{186}Re have been investigated. ^{170}Tm is found to have a β-ray end-point of $1{\cdot}00 \pm 0{\cdot}005$ Mev. with possible lower-energy groups at $0{\cdot}90 \pm 0{\cdot}015$, $0{\cdot}79 \pm 0{\cdot}025$, $0{\cdot}45 \pm 0{\cdot}05$ Mev. γ-rays with energies $82{\cdot}6 \pm 0{\cdot}5$, 205 ± 10, 430 ± 20 kev. were observed. The 430 kev. radiation is thought to be complex. ^{186}Re is found to have a β-ray end-point of $1{\cdot}090 \pm 0{\cdot}005$ Mev. with other groups at $0{\cdot}95 \pm 0{\cdot}015$ and $0{\cdot}64 \pm 0{\cdot}03$ Mev. Three γ-rays were observed, having energies 700 ± 50, 275 ± 8 and 133 ± 1 kev. The last is assumed to be a mean energy-value of the three low-energy γ-rays found by Cork, Shreffler and Fowler. Decay schemes are suggested for both isotopes.

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§ 1. INTRODUCTION

As a result of measurements made with a short magnetic lens β-ray spectrometer and by the use of absorption and coincidence techniques we have been able to suggest decay schemes for the radioactive isotope of thulium, ^{170}Tm, and the longer-lived active isotope of rhenium, ^{186}Re. The active samples were prepared in the low-energy pile at the Atomic Energy Research Establishment, Harwell, by slow neutron irradiation of thulium oxide (Tm_2O_3) for periods of one month and of metallic rhenium for periods of one week.

§ 2. ^{170}Tm

This isotope decays by β-emission to the ytterbium isotope ^{170}Yb with a half-life of approximately 127 days. It has previously been investigated by Bothe (1946), who obtained a value of 1 Mev. for the upper limit of the β-ray energy by absorption in aluminium and who stated that no γ-rays were emitted.

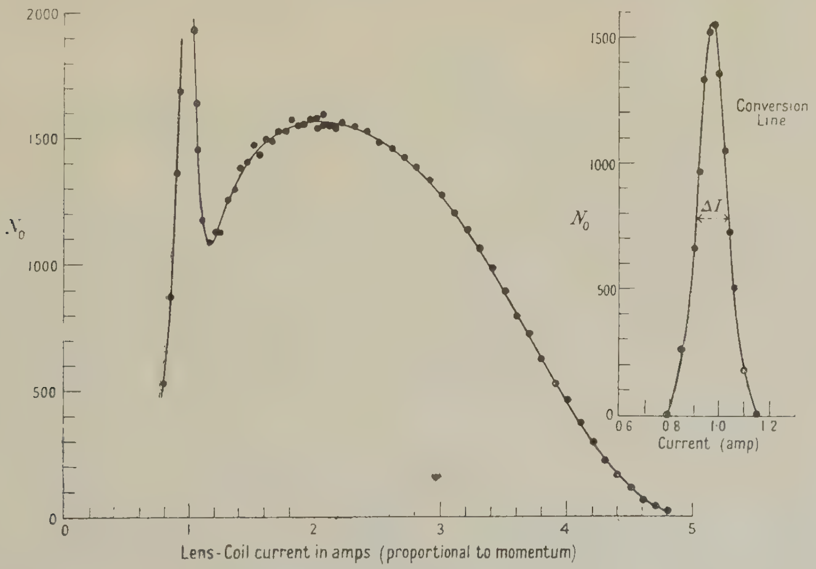

Figure 1. ^{170}Tm β-spectrum (counting rate normalized for equal momentum intervals).

Figure 1 shows a typical β-spectrum obtained with the spectrometer. A strong conversion line is apparent, corresponding to an electron energy of

72·1 ± 0·5 kev. The resolving power of the spectrometer is low. $\Delta I/I$ at half maximum on the conversion line is about 8%. Figure 2 is a curve showing the absorption of the γ-radiation in lead. It may be resolved to give γ-rays of energies 430 ± 20 kev. and 205 ± 10 kev., and an additional softer radiation whose energy lies below that of the K absorption edge of lead. Absorption measurements using brass absorbers (Figure 3) gave a value of 83 ± 2 kev. for the energy of this γ-ray. There seems little doubt that the conversion line (Figure 1) corresponds to this energy transition, and is due to the conversion in the L electron shell of ytterbium of a γ-ray of energy 82·6 ± 0·5 kev.

Figure 4 shows a Kurie plot of the continuous β-spectrum and gives a value for the upper limit of the β-ray energy of 1·00 ± 0·005 Mev. The shape of the Kurie plot indicates that the β-spectrum is complex and, as shown in Figure 4, it is possible to resolve it into four components giving additional β-spectra with end-points 0·90 ± 0·015 Mev., 0·79 ± 0·025 Mev., 0·45 ± 0·05 Mev. This procedure is, in general, untrustworthy, but in the present instance it seems justified since these energy values are consistent with the values obtained for the γ-ray energies. If correct, these β-ray energies show the possible existence of γ-rays of approximate energies 120, 350, 450 and 550 kev. in addition to the 82·6 and 205 kev. γ-rays. The first of these has not been found and, owing to the low intensity of the γ-radiation from the available sources, that part of the absorption curve corresponding to the three harder γ-rays could not be obtained with sufficient accuracy to determine whether the 430 kev. γ-ray was, in fact, complex.

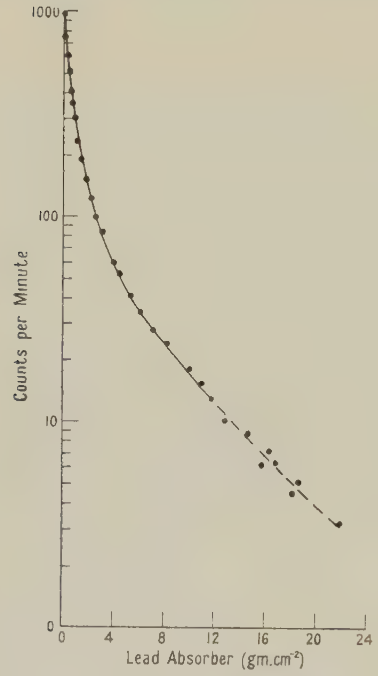

Figure 2. ^{170}Tm absorption of γ-radiation in lead.

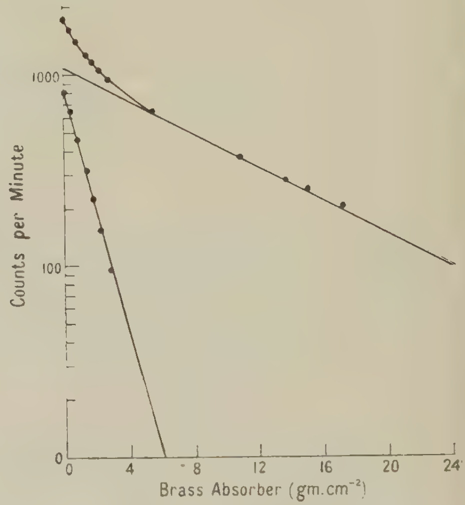

Figure 3. ^{170}Tm absorption of γ-radiation in brass.

From direct examination of the β-spectrum the number of L-conversion electrons was estimated as 4·2 ± 0·7% of the total disintegrations. When an approximate correction is made for absorption in the window of the Geiger counter the value 5·6 ± 1% is obtained. An estimate of this quantity was also made by observation of coincidences between β-rays and conversion electrons, the value obtained being 6·1 ± 0·5%. The two results are therefore in satisfactory agreement. An estimate of the number of K-conversion electrons was made by determining the K X-ray intensity, using an X-ray counter calibrated by means of a samarium source (^{153}Sm), where the ratio of X-ray intensity to β-particle intensity is known (Hill and Shepherd, private communication). This gave the number of K-conversion electrons as 5 ± 1% of the total disintegrations.

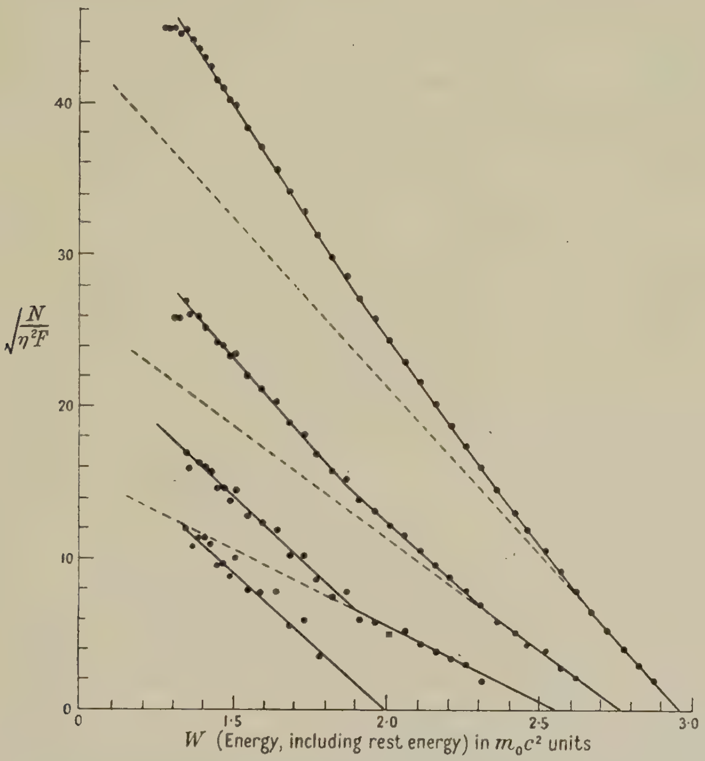

Figure 4. ^{170}Tm Kurie plot of continuous β-spectrum showing resolution into four possible components.

By the use of β- and γ-counters of known efficiency (Dunworth 1940), and by comparison of the β- and γ-counting rates given by the Tm source with those obtained using a ThC″ source, the following values were obtained for the intensities of the γ-rays. In each case the intensity is expressed as a percentage of the total number of disintegrations.

γ-ray (kev.)	82·6	205	430
Intensity (%)	25 ± 5	3·4 ± 0·3	0·6 ± 0·1

(The figure for the 82·6 kev. γ-ray includes the percentage of conversion electrons.) These values are satisfactorily confirmed by estimates of the intensities of the partial β-spectra obtained by the Kurie plot analysis. This is further evidence that the analysis is justified.

The proposed decay scheme is shown in Figure 5. As stated above, it was not possible to carry out a further resolution of the hard component of the γ-radiation. However, by absorption of $\gamma-\gamma$ coincidences it has been shown that the 82·6 kev. and 205 kev. γ-rays are not in coinidence but that each is in coincidence with a harder γ-radiation. The possible harder γ-rays are therefore included in the scheme.

By comparison of the ratios of the numbers of K- and L-conversion electrons, N_K/N_L, and of L-conversion electrons and γ-quanta, N_L/N_Q, with the values calculated by Hebb and Nelson (1940) it seems likely that the 82·6 kev. transition may be assigned to electric quadrupole radiation. It might, however, be magnetic dipole radiation. The following table shows the experimental values, the calculated values for electric multipole radiation with $l=1$, 2, 3, and also the calculated values for magnetic dipole radiation.

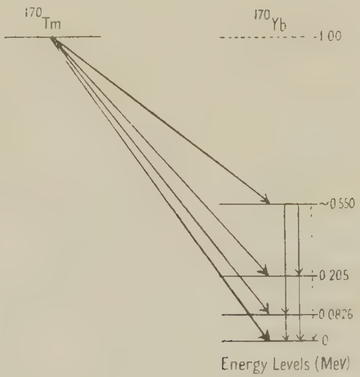

Figure 5. Suggested decay scheme for ^{170}Tm.

	Electric			Magnetic	Observed
l	1	2	3	1	
N_K/N_L	9	1·0	0·2	2·5	0·9 ± 0·25
N_L/N_Q	0·025	2·1	70	0·22	0·40 ± 0·15

Since ^{170}Yb is a stable even–even nucleus, the spin of its ground state is zero. Therefore it is presumed that the 82·6 kev. level has a spin of 2.

§ 3. ^{186}Re

This isotope has been investigated by Goodman and Pool (1947) and, more recently, by Cork, Shreffler and Fowler (1948). Prevously it was reported that no γ-rays were emitted in the disintegration, but this has been disproved by our results and by those of Cork *et al.* Using a 180°-type spectrometer in conjunction with a photographic plate, they observed conversion-electron lines from two, and possibly three, low-energy γ-rays. They also showed the presence of a harder γ-radiation by absorption measurements.

Slow neutron irradiation of rhenium gives rise to two activities. The shorter, which has a half-life of approximately 18 hours, has been assigned to ^{188}Re. This activity was allowed to decay before measurements were taken. The ^{186}Re activity was found to decay with a half-life of 91 hours, in good agreement with the results of Cork.

Figure 6 shows a typical β-spectrum with two groups of conversion electrons, one strong and one very weak. Measurements of the absorption in lead of the γ-radiation, Figure 7, demonstrate the presence of three components of energies :

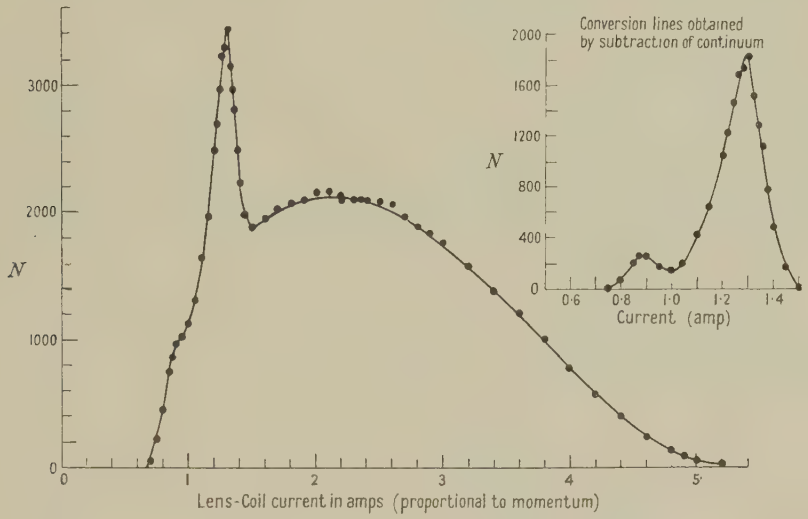

Figure 6. ^{186}Re β-spectrum (counting rate normalized for equal momentum intervals).

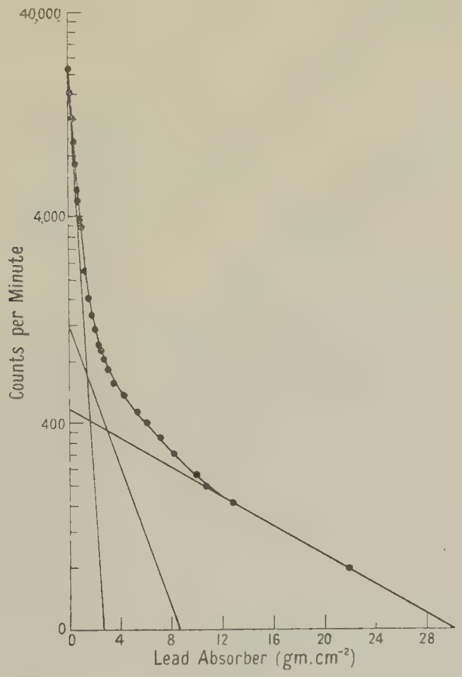

Figure. 7 ^{186}Re absorption of γ-radiation in lead.

700 ± 50 kev., 275 ± 8 kev., 131 ± 2 kev. The presence of the 275 kev. γ-ray was confirmed by spectrometer measurements of the energies of Compton electrons ejected from a 160 mg. cm^{-2} lead radiator.

A Kurie plot of the continuous β-spectrum is shown in Figure 8. It is possible to resolve this into three partial spectra and to estimate their energies and intensities as follows :

Energy (MEV.)	$1{\cdot}090 \pm 0{\cdot}005$	$0{\cdot}95 \pm 0{\cdot}015$	$0{\cdot}64 \pm 0{\cdot}03$
Intensity (%)	67 ± 1	30 ± 3	3 ± 4

The low energy β-transition corresponding to the 700 kev. γ-ray was not observed owing to the low intensity.

The conversion electron groups were at first assigned to the conversions in the K- and L-shells of osmium of a γ-ray of energy 133 ± 1 kev. However, Cork, Shreffler and Fowler have since shown, by observation of K-, L- and M-conversion groups, that there exist γ-rays having energies 122·7, 135·8 and, possibly, 137·5 kev. The resolving power of our spectrometer was not sufficiently high to permit the resolution of these conversion groups, nor, of course, was it possible by absorption measurements to resolve the corresponding γ-rays.

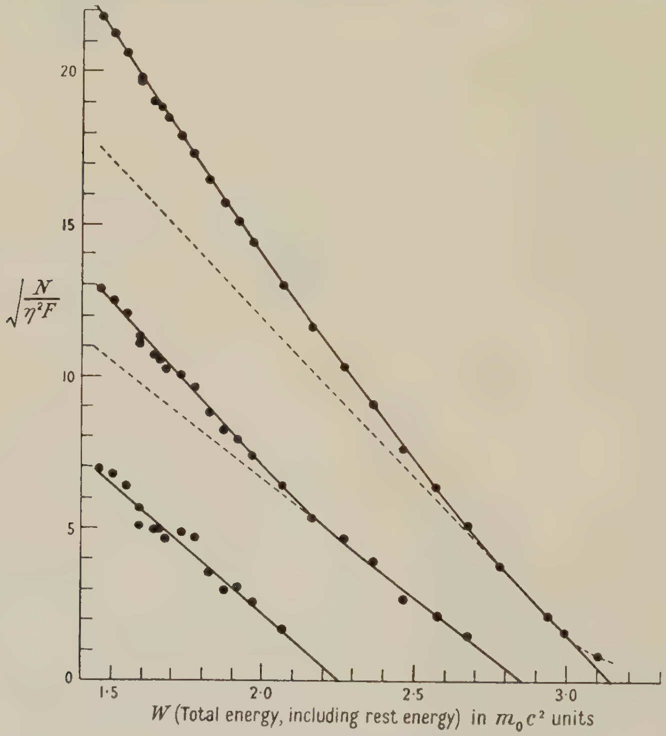

Figure 8. ^{186}Re Kurie plot of β-spectrum showing resolution into three components.

By inspection of the β-spectrum plot, the number of L-conversion electrons was estimated to be $6{\cdot}5 \pm 1\%$ of the total number of disintegrations. This is in agreement with the figure of $5{\cdot}0 \pm 0{\cdot}5\%$ obtained by coincidence experiments.

Experiments with calibrated counters gave the γ-ray intensities as

γ-ray (kev.)	133	275	700
Intensity (%)	37 ± 6	$2{\cdot}3 \pm 0{\cdot}2$	$0{\cdot}7 \pm 0{\cdot}1$

(The figure for the 133 kev. transition includes the percentage of conversion electrons.)

§ 4. DECAY SCHEME

Any decay scheme has to satisfy several conditions which are to some extent conflicting :

1. The relative intensities of the 122·7, 135·8 and 137·5 kev. γ-rays found by Cork must be such as to give a mean value for the energy which is consistent with our estimate.

2. Cork does not list the 137·5 kev. γ-ray as definite, but only "possible". This implies that, if it exists, it must be of very low intensity, very little converted, or both.

3. Any scheme must give values for the β-ray branching ratios which are not widely different from the values obtained by the Kurie plot analysis. In addition, the scheme must, of course, agree with all the energy values obtained.

The first possibility (Figure 9(*a*)) leads to the following conclusions :

(*a*) Total intensity of 122·7 kev. transition $\simeq 7\%$ of the total disintegrations.
(*b*) Total intensity of 135·8 kev. transition $\simeq 25\%$ of the total disintegrations.

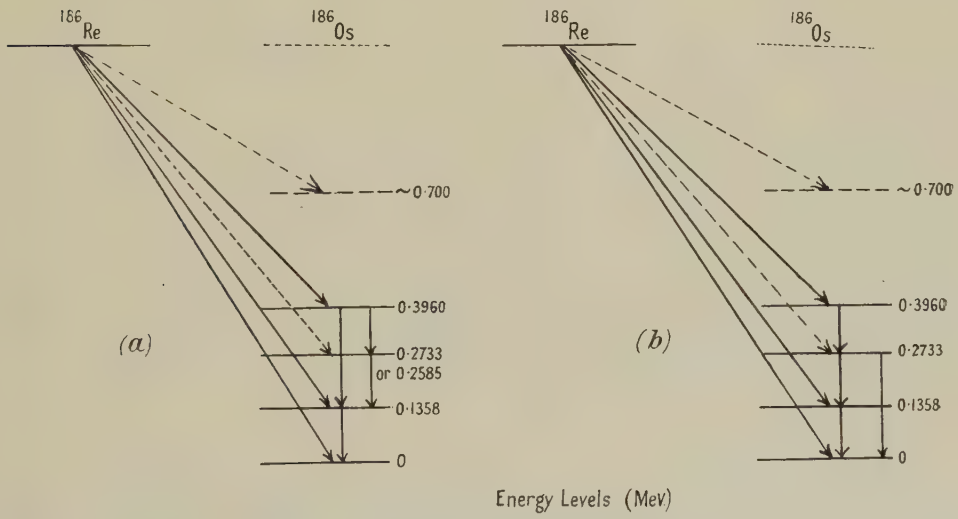

Figure 9. Possible decay schemes for ^{186}Re.

The β-ray branching ratios are thus 9% : 16% : 75%, which are to be compared with the experimental values of 3% : 30% : 67%. In addition, the scheme gives a value of 260 kev. for the γ-ray energy measured as 275 ± 8 kev. This seems unlikely, though possible.

The second scheme (Figure 9(*b*)), although not leading to perfect agreement, fits the facts more satisfactorily than the first. Calculations carried out as above lead to the results :

(*a*) Total intensity of 122·7 kev. transition 6·5% of total disintegrations.
(*b*) Total intensity of 135·8 kev. transition 28·5% of total disintegrations.
(*c*) Total intensity of 137·5 kev. transition 4% of total disintegrations.

We therefore obtain β-ray branching ratios of 6·5% : 24·5% : 69%. These are in considerably better agreement with the experimental values. Furthermore, the value given by the scheme for the γ-ray energy measured as 275 ± 8 kev. is now 273·3 kev.

It seems impossible by coincidence measurements to decide between these two schemes. The only deciding factor would be an accurate determination of the energy of the 275 kev. γ-ray. On the general grounds stated above it appears likely that Figure 9(*b*) gives the true representation of the decay scheme.

REFERENCES

Bothe, W., 1946, *Z. Naturforsch.*, **1**, 173.
Cork, J. M., Shreffler, R. G., and Fowler, C. M., 1948, *Phys. Rev.*, **74**, 1657.
Dunworth, J. V., 1940, *Rev. Sci. Instrum.*, **11**, 167.
Goodman, L. J., and Pool, M. L., 1947, *Phys. Rev.*, **71**, 288.
Hebb, M. H. and Nelson, E., 1940, *Phys. Rev.*, **58**, 486.

A Note on the ^{10}B(n, α)^{7}Li Reaction

By S. DEVONS
Cavendish Laboratory, Cambridge

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ABSTRACT. It is difficult to explain the observed preponderance of transitions to the excited state of ^{7}Li in the reaction ^{10}B (n, α) ^{7}Li in terms of the usual assumption that the ground state and first excited state of ^{7}Li form a $^2P_{3/2,\,1/2}$ doublet. In addition the doublet separation (0·48 Mev.) would be much larger than expected theoretically. If the excited state of ^{7}Li has total angular momentum other than 1/2, one would expect to find, in general, some angular correlation between the direction of emission of the α-particle, that of the subsequent γ-radiation and the polarization of the radiation. The correlations to be expected have been calculated on the assumption that the ground state of ^{7}Li is (3/2, odd), that of ^{10}B (3, odd), or (3, even), and that the compound nucleus ^{11}B has definite $J=5/2$ or 7/2. Results are given for excited states of ^{7}Li with total angular momentum 1/2, 3/2, 5/2 and 7/2 and both even and odd parities.

§ 1.

It is well established that in the disintegration of ^{10}B by slow neutrons two alpha-particle groups are emitted corresponding to the production of ^{7}Li in the ground state and in an excited state of energy between 0·45 and 0·50 Mev., the lower energy group being at least ten times as intense as the other (Boggild 1945, Gilbert 1948). An excited level of ^{7}Li of about the same energy has been observed in numerous reactions in which the ^{7}Li nucleus is the end product and it seems likely that the same level with an energy of 0·479 ± 0·002 Mev. according to the best measurements is involved in all cases*. On the assumption that there is in fact a single level of ^{7}Li in the region 0·45 – 0·50 Mev., it is tempting to suppose that this level together with the ground state form an "inverted" P doublet, $^2P_{3/2}$ (ground state) and $^2P_{1/2}$ (0·479 Mev. level), as predicted theoretically (Wigner and Feenberg 1941). This interpretation gains support from the recent experimental determination by Elliott and Bell (1948) of the upper limit for the life-time of this excited state of ^{7}Li (produced in ^{10}B (n, α) reaction) as 2×10^{-13} sec., which is consistent with a magnetic dipole transition between the two states in which the transition is attributed to the single odd 2p proton.

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* A complete list of the experimental results (up to 1947) is given by Rosenfeld (1948).

There are, however, serious theoretical difficulties in interpreting the large energy difference of 0·48 Mev. as a doublet splitting, and these difficulties have prompted the suggestion by Inglis (1948) that the actual doublet splitting is very much smaller than the observed energy difference between the ^{7}Li levels and that the ground state and the excited level each comprise closely spaced doublets, the former $^2P_{1/2,\,3/2}$ and the latter $^2F_{5/2,\,7/2}$. Evidence in favour of such a hypothesis comes from the recent measurements of the spin of ^{10}B (Gordy, Ring and Burg 1948) as 3 rather than 1 as previously supposed. The capture of a slow neutron by ^{10}B results, then, in a state of ^{11}B with spin 5/2 or 7/2 (or possibly, but unlikely, a mixture of states with appreciable components of each type), and the dissociation of the compound nucleus ^{11}B into ^{7}Li and an alpha-particle may take place to the F levels of ^{7}Li with smaller angular momentum for the alpha-particle than would be possible for transitions to the P levels. One could, in this way, interpret the observed preponderance of alpha-particles of lower energy, i.e. transitions to the excited state(s), since the energy available in the dissociation of ^{11}B is only 3·2 Mev. and one would expect the probability of dissociation to be quite sensitive to the magnitude of the angular momentum with which the alpha-particle is ejected. Table 1 shows the *lowest* angular momentum with which

Table 1. Angular Momentum Relations in the Reaction $^{11}B\,(p, \alpha)\,^7Li$.

^{7}Li state		Lowest radiation multipole to ground state (3/2, −)	^{10}B, even parity (+)		^{10}B, odd parity (−)	
Spin	Parity		$^{11}B^*$: 7/2, +	$^{11}B^*$: 5/2, +	$^{11}B^*$: 7/2, −	$^{11}B^*$: 5/2, −
1/2	+	Electric dipole	**4**	**2**	**3**	**3**
	−	Magnetic dipole and electric quadrupole	**3**	**3**	**4**	**2**
3/2	+	Electric dipole	**2,** 4	**2,** 4	**3,** 5	**1,** 3
	−	Magnetic dipole and electric quadrupole	**3,** 5	**1,** 3	**2,** 4	**2,** 4
5/2	+	Electric dipole	**2,** 4, 6	**0,** 2, 4	**1,** 3, 5	**1,** 3, 5
	−	Magnetic dipole and electric quadrupole	**1,** 3, 5	**1,** 3, 5	**2,** 4, 6	**0,** 2, 4
7/2	+	Magnetic quadrupole and electric octupole	**0,** 2, 4, 6	**2,** 4, 6	**1,** 3, 5, 7	**1,** 3, 5
	−	Electric quadrupole	**1,** 3, 5, 7	**1,** 3, 5	**0,** 2, 4, 6	**2,** 4, 6

The last four columns give the orbital angular momenta possible in the break up of the compound ^{11}B nucleus, to leave ^{7}Li in the state specified in the first two columns. The smallest angular momentum, shown in heavy type, has been used in the calculations. The fourth line (3/2, −) gives the values for the transition to the ground state of ^{7}Li.

the alpha-particle can be ejected, as determined by the conservation laws of total angular momentum and parity, for different assumed properties of the levels of ^{7}Li and for ^{10}B having either even or odd parity. (Theoretically even parity is more likely (Wigner and Feenberg 1941).) It should be noticed that in a particular case, if the state of ^{11}B is assumed to have definite J, the possible angular momenta of the components differ by two units so that the assumption that the lowest angular momentum component predominates should be fairly reliable with alpha-particles of such low energy.

The main objections to the assumption of close doublets for both levels is that, although estimates of the level position as derived from observation of different nuclear processes do range from 0·425 to 0·50 Mev.*, there is no indication of fine-structure of the levels in any single experiment. In particular, the high resolution available in the measurements of the gamma-radiation (of the order 1% in the experiments of Elliott and Bell (1948) and of Siegbahn (1946)) should be ample to reveal the smallest doublet splitting to be expected on theoretical grounds (of the order 0·01 Mev. for axial-dipole coupling and 0·1 Mev. for "Thomas" precession (Rosenfeld 1948)). It might be argued that transitions from or to *both* members of a doublet are unlikely in one particular mode of exciting the levels. Such an argument could be used, for example, if the excited state of ^{7}Li produced in a particular process was $^{2}F_{7/2}$, which could then radiate to the ground state $^{2}P_{3/2}$ by emission of electric quadrupole radiation, but for the transition to the $^{2}P_{1/2}$ member of the doublet, the lowest multipole would be magnetic octupole (2^3) or electric 2^4-pole, both of which would probably be of negligible intensity in comparison with the competing electric quadrupole. It would still remain difficult to understand why the $^{2}F_{5/2}$ state is not excited in some of the reactions, and for this state the two transitons to the $^{2}P_{3/2}$ and $^{2}P_{1/2}$ states would be mixed electric quadrupole plus magnetic dipole and electric quadrupole, respectively, which two should be of the same order of probability.

§ 2.

Apart then from any specific assumptions about the multiplet structure and precise spectral identification of the levels of ^{7}Li it is apparent that the assignment of spin 1/2 to the observed level (or levels) of ^{7}Li at about 480 kev. presents difficulties in relation both to the present limited theory and to the experimental data. One of the consequences of assuming that the spin of ^{7}Li is *not* 1/2 is that one would expect, in general, some angular correlation between alpha-particle and gamma-quantum in the process:

$$\begin{array}{l} {}^{10}\mathrm{B} + \mathrm{n} \rightarrow {}^{11}\mathrm{B}^* \rightarrow {}^{7}\mathrm{Li}^* + \alpha \\ \qquad\qquad\qquad\qquad\quad \downarrow \\ \qquad\qquad\qquad\qquad\quad {}^{7}\mathrm{Li} + \gamma \end{array}$$

both in relative intensity and in polarization. The magnitudes of such correlations have been calculated on the basis of the following simplifying approximations :—

(i) It has been assumed that J, the total angular momentum of the compound ^{11}B nucleus, is a good quantum number. In view of the low density of levels in such a light nucleus it seems reasonable to assume that the effect of a single level of the compound nucleus predominates and that therefore J is well defined.

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* Recent measurements by Rasmussen, Lauritsen and Lauritsen (1949) indicate the same energy in all cases.

(ii) The spins of both neutrons and ^{10}B nuclei are assumed to be randomly oriented. This represents the normal experimental conditions.

(iii) In the emission of the alpha-particles from the compound state it is assumed that the angular momentum of these particles is the lowest value consistent with conservation of angular momentum and parity (see Table 1).

(iv) The gamma-radiation is assumed to be of multipolarity not higher than electric quadrupole, in view of the observed upper limit for the life-time. When both magnetic dipole and electric quadrupole transitions between two states can occur, the possibility that the two effects are of comparable amplitude has been considered, and the transition is treated in such cases as a mixed one, with an unknown complex coefficient introduced to represent the relative magnitude and phase of the two components. The possible radiation multipoles that can arise are, as shown in Table 1, electric dipole, electric quadrupole, and mixed electric quadrupole with magnetic dipole. The radiation multipoles shown are all based on the assumption of $J=3/2$ and odd parity for the ground state of ^{7}Li.

(v) All the results are given for a frame of references in which the centre of gravity of the system ($^7\mathrm{Li}+\alpha$) is at rest. Corrections to allow for the motion of the radiating ^{7}Li nucleus would, in part, depend on the variation of efficiency with energy of the gamma-detecting apparatus, and have not therefore been included (cf. Devons and Hine 1949).

§ 3.

Calculations have been made for the following cases:

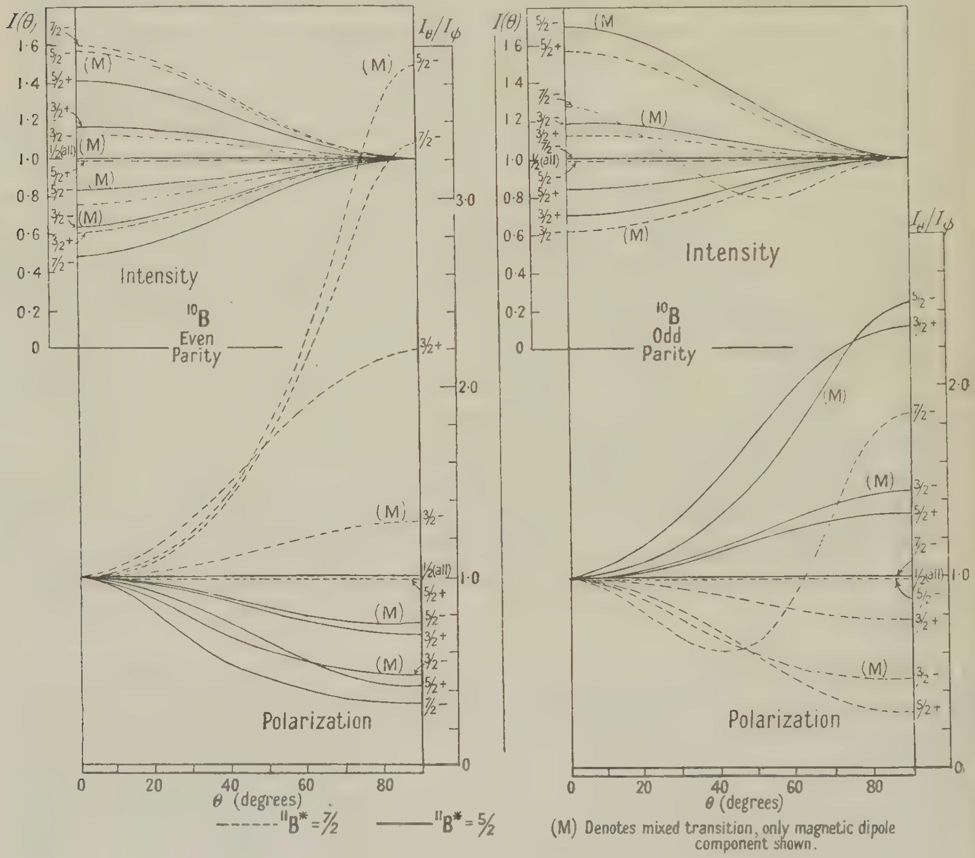
(*a*) The ground state of ^{10}B having either even or odd parity.

(*b*) The compound ^{11}B nucleus having $J=5/2$ or $7/2$.

(*c*) States of ^{7}Li with spin up to $7/2$ and odd parity and up to $5/2$ and even parity. Higher spin values lead to higher multipole transitions than specified in (iv) above.

The results of the calculations are given in Table 2, and illustrated in the accompanying diagram. The intensity of the radiation $I(\theta)$ is given as a function of the angle between the directions of observation of the emitted gamma-radiation and the preceding alpha-particle. $\theta=0$ corresponds to the direction of emission of the latter. The degree of polarization, which is of course a function of the angle θ, is expressed as a ratio of the intensity, I_θ, of the components polarized (i.e. electric vector) in the plane containing the directions of the observed alpha-particle and gamma-quantum, and the intensity, I_φ, in the perpendicular direction. This ratio does not exhaust all the possible information that may be obtained about the polarization, since, in general, the radiation is an incoherent mixture of some linearly and some elliptically polarized components. If however, as is normally the practice (see, e.g., Deutsch and Metzger 1948) the polarization is measured using a device which has no specified axial vector, such as a magnetic field, associated with it, only the ratio I_θ/I_φ will be significant in interpreting the observations.

It is apparent from Table 2 that if the excited state of ^{7}Li has spin other than $1/2$, then in general the angular correlation of intensity and polarization will be quite marked, and probably of the form $1+a\cos^2\theta$. The main ambiguity in a detailed theoretical interpretation of an observed angular correlation is due to the approximate constancy of the correlation functions if one makes the change from even to odd parity of ^{10}B, together with the change from one of the values 5/2 or 7/2 for ^{11}B to the other. It should also be noticed that if the gamma-radiation is observed to be isotropic with respect to the direction of emission of the alpha-particle, this does not provide conclusive evidence for a value 1/2 for the spin of the excited state. Whilst the isotropy in other cases is only approximate (due to neglect of higher angular momentum components of the ejected alpha-particle), the departures from isotropy may well be too small to be detected.



α-particle, γ-quantum correlation in the ^{10}B (n, α) ^{7}Li* reaction.

Note added in proof. Since the paper was written, calculations of a similar nature, though covering a different range of possibilities, have been published by B. T. Feld (*Phys. Rev.*, 1949, **75**, 1618). Feld considers intensity correlation only, and assumes that the alpha-particle is emitted with one unit of orbital angular momentum. Explicit mention is not made of the parity of ^{10}B, although it appears to have been assumed even. Mixed electric-magnetic transitions are not considered. In two cases where comparison is possible, the calculations are in agreement. A note has also been published recently by S. S. Hanna and D. R. Inglis (*Phys. Rev.*, 1949, **75**, 1767) in which the levels of ^{7}Li are discussed in terms of possible coupling schemes.

Table 2. Angular Correlation and Polarization in the Reaction $^{10}\mathrm{B}(\mathrm{p},\alpha)\,^{7}\mathrm{Li}^*$.

Excited state of ^{7}Li		^{10}B even parity (+)		^{10}B odd parity (−)	
		^{11}B*, 7/2, +	^{11}B*, 5/2, +	^{11}B*, 7/2, −	^{11}B*, 5/2, −
1/2	+	Isotropic in all cases			
	−				
3/2	+	$1+2/11\cos^2\theta$ $[1-4/13\sin^2\theta]$	$1-3/8\cos^2\theta$ $[1+6/5\sin^2\theta]$	$1-5/17\cos^2\theta$ $[1+1{\cdot}32\sin^2\theta]$	$1+1/8\cos^2\theta$ $[1-2/9\sin^2\theta]$
	−	$1-\cos^2\theta\,\frac{6(1+\gamma)}{(17+36\alpha+12\gamma)}$ $\left[\frac{11+36\alpha+24\gamma-48\gamma\cos^2\theta}{23+36\alpha-12\cos^2\theta(1+2\gamma)}\right]$	$1+\cos^2\theta\,\frac{(1+6\gamma)}{(8+20\alpha-2\gamma)}$ $\left[\frac{9-20\alpha-4\gamma+8\gamma\cos^2\theta}{7+20\alpha+2\cos^2\theta(1+2\gamma)}\right]$	$1+\cos^2\theta\,\frac{2(1+6\gamma)}{(11+28\alpha-4\gamma)}$ $\left[\frac{13+28\alpha-8\gamma(1-2\cos^2\theta)}{9+28\alpha+4\cos^2\theta(1+2\gamma)}\right]$	$1-\cos^2\theta\,\frac{15(1+6\gamma)}{2(20+42\alpha+15\gamma)}$ $\left[\frac{25+84\alpha+60\gamma(1-2\cos^2\theta)}{55+84\alpha-30\cos^2\theta(1+2\gamma)}\right]$
5/2	+	$1+17/41\cos^2\theta$ $[1-17/29\sin^2\theta]$	Isotropic	$1-1/7\cos^2\theta$ $[1+1/3\sin^2\theta]$	$1+4/7\cos^2\theta$ $[1-8/11\sin^2\theta]$
	−	$1-\cos^2\theta\,\frac{(1-0{\cdot}4\alpha-9/2\gamma)}{(7+5{\cdot}1\alpha-3/2\gamma)}$ $\left[\frac{3(1+0{\cdot}92\alpha-1/2\gamma+\gamma\cos^2\theta)}{4+2{\cdot}36\alpha-\cos^2\theta(1-0{\cdot}4\alpha-3/2\gamma)}\right]$	$1+\cos^2\theta\,\frac{(1-0{\cdot}4\alpha-4{\cdot}8\gamma)}{(1{\cdot}75+1{\cdot}74\alpha+1{\cdot}6\gamma)}$ $\left[\frac{1+0{\cdot}5\alpha+1{\cdot}09\gamma-2{\cdot}18\gamma\cos^2\theta}{0{\cdot}27+0{\cdot}79\alpha+\cos^2\theta(0{\cdot}73-0{\cdot}3\alpha-1{\cdot}1\gamma)}\right]$	$1+\cos^2\theta\left(\frac{1+0{\cdot}61\alpha-4{\cdot}5\gamma-1{\cdot}18\alpha\cos^2\theta}{1{\cdot}43+2{\cdot}78\alpha+1{\cdot}5\gamma}\right)$ $\left[\frac{1+0{\cdot}66\alpha-0{\cdot}18\alpha\cos^2 2\theta-0{\cdot}9\gamma\cos 2\theta}{0{\cdot}41+0{\cdot}82\alpha+\cos^2\theta(0{\cdot}59-0{\cdot}34\alpha-0{\cdot}9\gamma)}\right]$	Isotropic
7/2	−	$1-1/2\cos^2\theta$ $[(3{\cdot}08-2{\cdot}08\cos^2\theta)^{-1}]$	$1+3/5\cos^2\theta$ $[(0{\cdot}3+0{\cdot}7\cos^2\theta)^{-1}]$	Isotropic	$1-1{\cdot}06\cos^2\theta+1{\cdot}35\cos^4\theta$ $\left[\frac{1-2{\cdot}12\cos^2\theta+2{\cdot}12\cos^4\theta}{0{\cdot}54+0{\cdot}46\cos^2\theta}\right]$

The first line in each case represents the intensity correlation; the second line in square brackets, the polarization ratio I_θ/I_φ. $\alpha=\beta^2$ and $\gamma=\beta\sin\delta$ where $\beta e^{i\delta}$ is used to denote the relative amplitude and phase of magnetic dipole to electric quadrupole components in a mixed transition. The results plotted in the figure are obtained by putting $\alpha=\gamma=0$ (pure magnetic dipole). The case of pure quadrupole radiation is obtained by retaining only the terms containing α.

REFERENCES

Bøggild, J., 1945, *Det. Kgl. Dansk. Vid. Sels. Math-Fys. Medd.*, **23**, 4.
Devons, S., and Hine, M. G. N., 1949, *Proc. Roy. Soc.* A, in the press.
Deutsch, M., and Metzger, F., 1948, *Phys. Rev.*, **74**, 1542.
Elliott, L. G., and Bell, R. E., 1948, *Phys. Rev.*, **74**, 1869.
Gilbert, C. W., 1948, *Proc. Camb. Phil. Soc.*, **44**, 447.
Gordy, W., Ring, H., and Burg, A. B., 1948, *Phys. Rev.*, **74**, 1191.
Inglis, D. R., 1948, *Phys. Rev.*, **74**, 1876.
Rosenfeld, L., 1948, *Nuclear Forces* (Amsterdam : North Holland Publ. Co.), § 17.5.
Rasmussen, V. K., Lauritsen, C. C. and Lauritsen, T., 1949, *Phys. Rev.*, **75**, 199.
Siegbahn, K., 1946, *Ark. Mat. Astr. Fys.*, **34B**, No. 6.
Wigner, E. P., and Feenberg, E., 1941, *Rep. Prog. Phys.*, **8**, 274 (London : Physical Society).

The Neutrons emitted in the Bombardment of ^{10}B and ^{11}B by Deuterons

By W. M. GIBSON
Cavendish Laboratory, Cambridge

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ABSTRACT. The photographic plate method has been used in an investigation of the neutrons from the (dn) reactions in ^{10}B and ^{11}B, separated isotopes being used in the targets.

The reaction ^{10}B(dn)^{11}C gives neutron groups at 6·70 Mev. and 4·85 Mev. ; these give an energy release for the transition to the ground state of 6·59 ± 0·10 Mev., and show the existence of a level in ^{11}C at 2·02 ± 0·10 Mev.

The reaction ^{11}B(dn)^{12}C gives well-defined neutron groups at 13·51 Mev., 9·40 Mev. and 4·55 Mev., with a continuous distribution up to 6 Mev. and a small peak at 6·4 Mev. The 13·51 Mev. group gives an energy release of 13·92 ± 0·15 Mev. for the transition to the ground state ; the other groups confirm the existence of levels in ^{12}C at 4·47 ± 0·10 Mev., 9·72 ± 0·15 Mev. and less certainly 7·7 Mev., while the continuous distribution is due to the reaction ^{11}B(dn)3α.

§1. INTRODUCTION

The energy spectrum of the neutrons produced by the bombardment of boron with deuterons has been studied by Powell (1943). He showed that the greater number of neutrons emitted were in two groups, of energies 4 Mev. and 9 Mev. ; there were also some neutrons in groups at 13 Mev. and 6 Mev.

Normal boron contains 80% of the isotope ^{11}B and 20% of the isotope ^{10}B. The energy release in the reaction

$$^{11}_{5}\mathrm{B} + ^{2}_{1}\mathrm{D} \rightarrow ^{12}_{6}\mathrm{C} + ^{1}_{0}\mathrm{n}$$

is 13·78 Mev. according to the mass-values quoted by Bethe (1947), while that for the reaction

$$^{10}_{5}\mathrm{B} + ^{2}_{1}\mathrm{D} \rightarrow ^{11}_{6}\mathrm{C} + ^{1}_{0}\mathrm{n}$$

is 6·53 Mev. Powell's 13 Mev. and 9 Mev. groups are therefore attributed to the disintegration of ^{11}B, the ^{12}C nucleus being formed in its ground state and in an excited state at 4 Mev., respectively. The 6 Mev. and 4 Mev. groups might be due to further excited states of ^{12}C, for which there is some evidence, or they might be due to the disintegration of the less abundant isotope ^{10}B.

In order to obtain more definite evidence about the excited states of ^{12}C and ^{11}C, studies of the neutron spectra from deuteron bombardment of the separated boron isotopes have been made and compared with that from a normal boron target. The photographic plate technique has been applied to the study of neutron spectra by Powell (1940, 1943), Richards (1941), Peck (1948) and others; it has been used in the work discussed here, the procedure being similar to that described by Green and Gibson (1949).

§2. EXPERIMENTAL TECHNIQUE

The specimens of the separated isotopes were in the form of thin deposits on the copper sheet which had been used as collector in the electromagnetic separator. The thicknesses were such that the energy loss of a deuteron passing through the ^{11}B layer was of the order of 120 kev., and through the ^{10}B layer 30 kev.; the separation of the isotopes was practically complete. The specimens were mounted on water-cooled brass plates to form targets 14 mm. in diameter, and were bombarded with a 70 μa. deuteron beam of mean energy 930 kev., from the Cavendish Laboratory 1 MV. equipment. A thick target of unseparated boron was bombarded under similar conditions.

Ilford C.2 plates, with emulsion thicknesses varying from 250μ to 290μ, were held in light-tight boxes so that they received at grazing incidence the neutrons emitted from the target at right angles to the direction of the deuteron beam. In the investigation of (dn) reactions some neutrons from the reaction

$$^{2}_{1}\mathrm{D}+^{2}_{1}\mathrm{D}\rightarrow{}^{3}_{2}\mathrm{He}+^{1}_{0}\mathrm{n}+3{\cdot}23\ \text{MeV.}$$

(Livesey and Wilkinson 1948) are always found, because of deuteron contamination of the target. Neutrons emitted at 90° to the beam were chosen because at this angle the target thickness has little effect, and the separation of the D + D group from the 4 MeV. B + D group is better than in the forward direction.

The distance of the plates from the target, and the exposure time, were chosen for each target to give a convenient number of tracks per unit area of emulsion. This was made much easier, and trial irradiations were made unnecessary, by the use of a high-pressure hydrogen ionization chamber; with this it was possible to estimate the neutron flux from each target at the beginning of the irradiation. The exposure times varied between 1 and 2 hours and the distances between 6 and 20 cm.

The plates were processed by the method due to Dilworth, Occhialini and Payne (1948), and were examined under a magnification of 1,100. Proton tracks forming "knock-on" angles of less than 19·5° with the neutron beam were measured, and the corresponding neutron energies calculated in the way described by Gibson and Livesey (1948). Tracks corresponding to neutrons of energy less than 4·0 MeV. were neglected for all but about a tenth of the area searched in each plate.

§3. NEUTRON SPECTRA

For each of the three spectra, the number of observed tracks in each energy interval of 0·2 MeV. was plotted as a function of neutron energy. To obtain the actual distribution of neutron number with energy, it was necessary to correct these spectra for two effects: the variation of neutron–proton scattering cross section with energy (Sleator 1947), and the variation with energy of the fraction of the proton tracks which would be expected to remain within the emulsion.

This fraction was calculated for each neutron energy by the formula of Gibson and Livesey (1948), the appropriate values of the emulsion thickness and the maximum allowed knock-on angle being used. The combined correction factor for cross section and escape is shown in Figure 1, for an emulsion thickness of 250μ and a knock-on angle limit of 19·5°. The factor is normalized to unity at 7 MeV., so that in the corrected spectra of Figures 2, 3 and 4 the ordinates around this energy represent actual numbers of tracks measured, while being proportional to neutron numbers at all energies. The effect of the variation of the correction factor is shown by the standard deviations indicated on the corrected spectra.

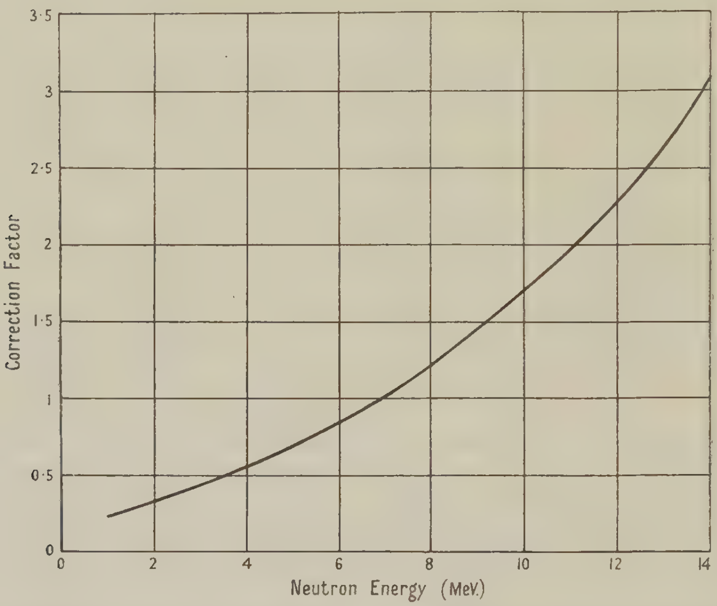

Figure 1. Combined correction for escape and n–p scattering cross section, for a 250 μ emulsion and a " knock-on " angle limit of 19·5°. The correction factor is normalized to unity at 7 MeV.

(i) *Discussion of Neutron Spectrum from ^{10}B.*

It is clear that the groups centred on 6·70 MeV. and 4·85 MeV. (see Figure 2) must be due to the reaction

$$^{10}_{5}B + ^{2}_{1}D \rightarrow ^{11}_{6}C + ^{1}_{0}n.$$

No other element could give either of these groups if present as contamination of the target, and the almost complete absence of ^{11}B is shown by the smallness of the 9·4 MeV. group. In particular, comparison of the relative sizes of the 9·4 MeV. and 4·5 MeV. groups in the ^{11}B spectrum (Figure 3) with those of the 9·4 MeV. and 4·85 MeV. groups in the ^{10}B spectrum shows that the last-mentioned group could not be due to ^{11}B contamination even if its mean energy fitted. The unduly low neutron energies (12·2 MeV. and 12·6 MeV.) obtained from the two long tracks in the ^{10}B spectrum are explained by assuming that the ^{11}B contamination was not on the ^{10}B target itself but on the brass holder beside it; this would introduce errors in the calculation of the neutron energy from the proton energy. The peak between 2 MeV. and 3 MeV. in Figure 2 is due to D + D neutrons.

If we assume that the thickness of the target was 30 kev. and the mean energy of the bombarding deuterons therefore 915 kev., we obtain values of $6\cdot59 \pm 0\cdot10$[*] Mev. and $4\cdot57 \pm 0\cdot10$[*] Mev. for the Q values corresponding to the two groups (we neglect the variation of the excitation function between 900 kev. and 930 kev.). The masses quoted by Bethe (1947) for $^{10}_{5}B$, $^{2}_{1}D$, $^{11}_{6}C$ and $^{1}_{0}n$ give a Q value of 6·53 Mev., so we must assume that the two groups are produced by the formation of ^{11}C in its ground state and in an excited state at $2\cdot02 \pm 0\cdot10$[*] Mev. There is as yet no other evidence for such a state, but these results appear conclusive.

(ii) *Discussion of Neutron Spectrum from ^{11}B*

Figure 3 shows the neutron spectrum from the ^{11}B target. The groups centred on 13·51 Mev., 9·40 Mev. and 4·55 Mev. must be due to the reaction

$$^{11}_{5}B + ^{2}_{1}D \rightarrow ^{12}_{6}C + ^{1}_{0}n.$$

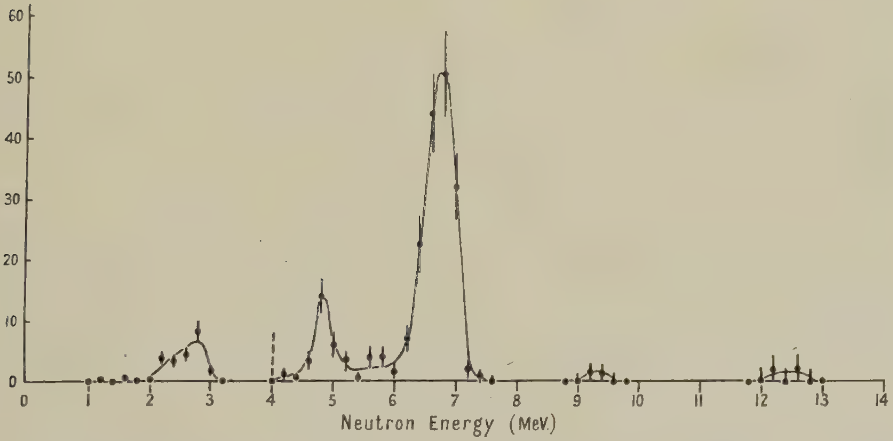

Figure 2. Neutron spectrum from ^{10}B target, from 285 tracks. Each ordinate is proportional to the number of neutrons in an energy interval of 0·2 Mev., and in the region of 7 Mev. is equal to the actual number of proton tracks observed in such an interval.

If we assume a target thickness of 120 kev. and use the known excitation function for the reaction (J. V. Jelley, private communication) we obtain an effective mean bombarding energy of 875 kev.; this gives Q values for the three groups of $13\cdot92 \pm 0\cdot15$ Mev., $9\cdot45 \pm 0\cdot15$ Mev. and $4\cdot20 \pm 0\cdot10$ Mev, respectively. The masses of $^{11}_{5}B$, $^{2}_{1}D$, $^{12}_{6}C$ and $^{1}_{0}n$ quoted by Bethe (1947) give a Q value of 13·78 Mev., so these three groups may be assumed to be due to the formation of ^{12}C in its ground state and in levels at $4\cdot47 \pm 0\cdot10$ Mev. and $9\cdot72 \pm 0\cdot15$ Mev. The existence of the lower level has been deduced by many workers, including Holloway and Moore (1940). Independent evidence for the higher level is in the work of Stuhlinger (1939), who observed the onset of slow neutron production in the reaction $^{9}Be(\alpha n)^{12}C$ at an α-energy corresponding to a level at 9·5 Mev., and of Fulbright and Bush (1948) who studied the inelastic scattering of protons by carbon and inferred the existence of a level at $9\cdot7 \pm 0\cdot6$ Mev.

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[*] The uncertainties of measurement and the uncertainty of the range–energy relation contribute roughly equally to the error in each Q value; each alone would give an error of ± 0·07 Mev. The uncertainties of measurement for the two groups are almost independent, but that of the range–energy relation is not. The probable error of the difference of the Q values is therefore almost entirely due to the uncertainties of measurement, which give 0·10 Mev.

The reaction

$$^{11}_{5}\mathrm{B} + ^{2}_{1}\mathrm{D} \rightarrow 3\alpha + \mathrm{n}$$

should have a Q value of 6·44 MeV.; calculation shows that the maximum energy which a 90° neutron from this reaction can have is 6·66 MeV., when the deuteron energy is 0·93 MeV. There will therefore be a continuous spread of neutrons of all energies up to 6·66 MeV., the exact shape of the distribution depending on the detailed mechanism of the reaction.

The spectrum of Figure 3 shows just such a continuous distribution (obscuring the D + D group which would appear between 2 MeV. and 3 MeV., as in Figure 2). The small peak at 6·4 MeV. might be due to this reaction, but is more probably due to a genuine level in ^{12}C at 7·7 MeV. Holloway and Moore (1940) give some evidence for such a level, and many workers have found gamma-rays corresponding to transitions to the ground state from a level in this region. This peak cannot be due to ^{10}B contamination since its mean energy is wrong. It may be mentioned that both the 7·7 MeV. and the 9·7 MeV. levels in ^{12}C are above the threshold for disintegration into three alpha-particles (7·34 MeV.).

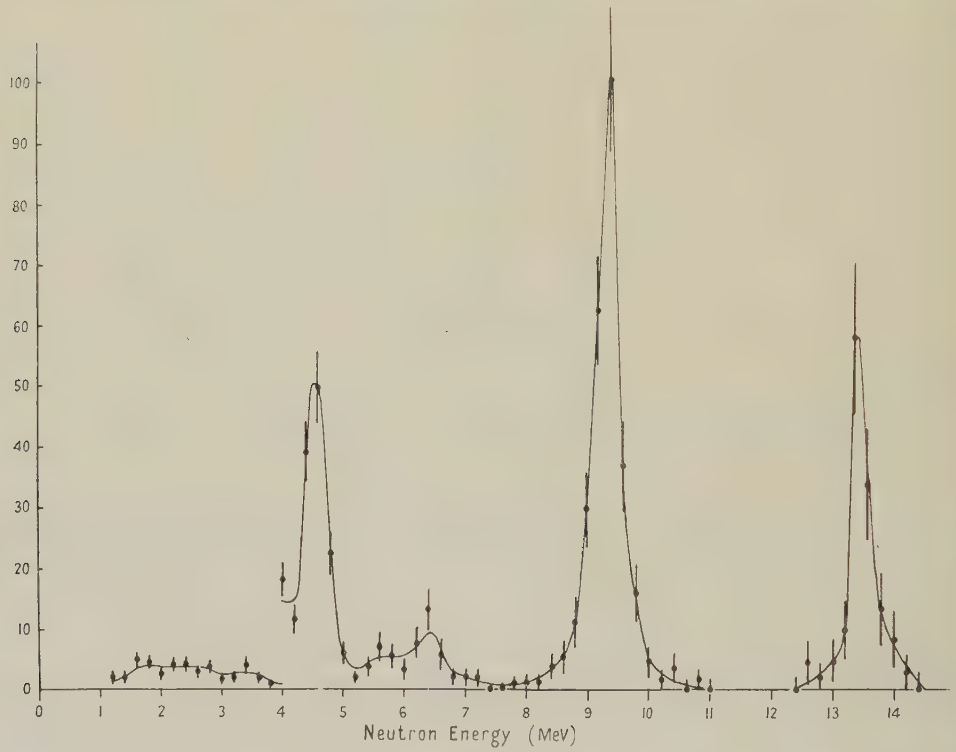

Figure 3. Neutron spectrum from ^{11}B target, from 663 tracks. For ordinate scale see Figure 2.

(iii) *Neutron Spectrum from Unseparated Boron*

The spectrum of Figure 4 agrees with that of Powell (1943), and is mainly of interest for the size and mean energy of the group between 6 and 7 MeV. This is much larger in proportion to the 9·4 MeV. group than in the ^{11}B spectrum; the fact that it is partly due to ^{10}B is shown also by its mean energy, which is 6·5 MeV.

compared with 6·4 Mev. for the small peak in the ^{11}B spectrum and 6·7 Mev. for the group from ^{10}B. Otherwise the unseparated-boron spectrum is very similar to that from ^{11}B.

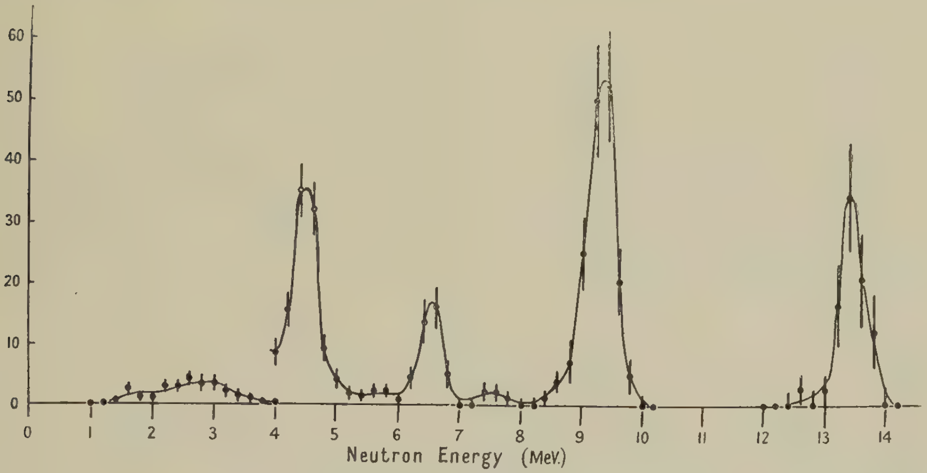

Figure 4. Neutron spectrum from unseparated boron target, from 447 tracks. For ordinate scale see Figure 2.

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REFERENCES

Bethe, H., 1947, *Elementary Nuclear Theory* (New York : John Wiley and Sons), p. 124.
Dilworth, C. C., Occhialini, G. P. S., and Payne, R. M., 1948, *Nature, Lond.*, **162**, 102.
Fulbright, H. W., and Bush, R. R., 1948, *Phys. Rev.*, **74**, 1323.
Gibson, W. M., and Livesey, D. L., 1948, *Proc. Phys. Soc.*, **60**, 523.
Green, L. L., and Gibson, W. M., 1949, *Proc. Phys. Soc.* A, **62**, 407.
Holloway, M. G., and Moore, B. L., 1940, *Phys. Rev.*, **58**, 847.
Livesey, D. L., and Wilkinson, D. H., 1948, *Proc. Roy. Soc.* A, **195**, 123.
Peck, R. A., 1948, *Phys. Rev.*, **73**, 947.
Powell, C. F., 1940, *Nature, Lond.*, **145**, 155 ; 1943, *Proc. Roy. Soc.* A, **181**, 344.
Richards, H. T., 1941, *Phys. Rev.*, **59**, 796.
Sleator, W., 1947, *Phys. Rev.*, **72**, 207.
Stuhlinger, E., 1939, *Z. Phys.*, **114**, 185.

Three-Dimensional Design of Synchrotron Pole-Faces

By C. ROBINSON

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MS. received 6*th April* 1949

ABSTRACT. An analytical method for the design of a synchrotron pole-face is outlined, the method taking full account of the cylindrical symmetry. Relaxation methods are employed to determine the exact size and shape of the lips correcting for fringing, and are also used to give a final check on the characteristics of the magnetic field. Practical examples of these methods are then given.

§ 1. INTRODUCTION

Two recent notes (Davy 1948, Goward and Wilkins 1948) have considered the design of a synchrotron pole-face. This suggests that the methods used by the author may be of interest to other workers in this field. The purpose of their designs has been to produce a field between the poles such that, using cylindrical polar coordinates, the value n given by

$$n = -(r/H_z)(\partial H_z/\partial r) \qquad \ldots\ldots(1)$$

is almost constant and has a value of about 0·7, H_z being the z-component of the field. The contours of the pole-faces have been deduced in two dimensions by neglecting the cylindrical symmetry and by a procedure which amounts to defining a magnetic potential distribution which does not satisfy Laplace's equation. A three-dimensional design in which the potential distribution does satisfy Laplace's equation was given by Coggeshall and Muskat (1944) and independently by Bartlett (1943), but, like that given by Davy, the value of n defined by (1) is constant only on the plane of symmetry, and is difficult to determine elsewhere. The first object of this paper is therefore to show how to define a magnetic potential distribution which satisfies Laplace's equation and which has an "almost constant" value of n over the whole area of the pole cross section as distinct from an "exactly constant" value of n along the plane of symmetry.

§ 2. THE DESIGN OF THE PROFILE

Goward and Wilkins have remarked that a constant value of n over the whole field is impossible. This is clearly true because the potential distribution which gives

$$H_z = kr^{-n}, \qquad \ldots\ldots(2)$$

where k and n are constants, does not satisfy Laplace's equation. The latter, with cylindrical symmetry, has the form

$$\frac{\partial^2 V}{\partial r^2} + \frac{1}{r}\frac{\partial V}{\partial r} + \frac{\partial^2 V}{\partial z^2} = 0. \qquad \ldots\ldots(3)$$

However, all expressions of the form

$$V = \mathscr{C}_0(kr)\sinh(kz) \qquad \ldots\ldots(4)$$

(where $\mathscr{C}_0(x)$ is a Bessel function of zero order of the first or second kind and k has any value) are solutions of the equation, have symmetry about $z=0$, and have zero value on $z=0$. The problem is, then, to choose a linear combination of terms of the type given in (4) such that the value of n defined by

$$n=-\left(r\Big/\frac{\partial V}{\partial z}\right)\left(\frac{\partial^2 V}{\partial r\,\partial z}\right), \qquad \ldots\ldots(5)$$

is "almost constant" over a prescribed cross section.

It is convenient to take as unit of distance the mean value of r in the area under consideration. Then a potential distribution given by (4) gives at $r=1$ values of n defined by

$$n=\{k\mathscr{C}_1(k)\}/\mathscr{C}_0(k), \qquad \ldots\ldots(6)$$

and thus for any prescribed n value at this point it is sufficient for k to be any root of equation (6). If the potential distribution is given by a linear combination of terms of the type (4), namely,

$$V=\Sigma_s\{A_s\mathscr{C}_0(k_s r)\sinh(k_s z)\}, \qquad \ldots\ldots(7)$$

then the appropriate value of n is

$$n=\frac{\Sigma_s\{A_s k_s^2 r\,\mathscr{C}_1(k_s r)\cosh(k_s z)\}}{\Sigma_s\{A_s k_s\mathscr{C}_0(k_s r)\cosh(k_s z)\}}. \qquad \ldots\ldots(8)$$

At the point $r=1$, $z=0$ this reduces to

$$n=\frac{\Sigma_s\{A_s k_s^2\mathscr{C}_1(k_s)\}}{\Sigma_s\{A_s k_s\mathscr{C}_0(k_s)\}}, \qquad \ldots\ldots(9)$$

and if the constants k_s are solutions of equation (6) for the prescribed n (say n') then this reduces to

$$n=\frac{\Sigma_s\{A_s B_s n'\}}{\Sigma_s\{A_s B_s\}}\equiv n'. \qquad \ldots\ldots(10)$$

That is to say, if each of the terms of (7) individually has the prescribed n value at the point $r=1$, $z=0$, then the expression (7) also has the prescribed value regardless of the values of A_s. These s arbitrary constants can then be chosen so that n has its prescribed value at s other points, and in this way a potential distribution is defined which gives a prescribed n value over an area, and not merely on the line $z=0$.

§ 3. THE EXTREMITIES OF THE PROFILE

The remaining problem is the design of a lip correcting for fringing. Relaxation methods can be employed for this purpose, this being a distinct improvement on methods employed hitherto which seem to have been based merely on a qualitative rather than a quantitative basis. The method suggested is as follows, reference being made to Figure 1. Let ABCD be the theoretical pole profile and suppose it must be terminated at a radius $r=r_1$, on the line EB. Let HCG be the greatest radius for which the n values have been specified. A relaxation mesh is then constructed to terminate on HG, GB, BE and HK. The boundary values of the potential are all known except on GB (it being assumed that plausible values are given on the boundary joining E to K, at such a distance that any error in them does not affect the values of n in the important area). In particular the values of the potential V and of the potential gradient $\partial V/\partial r$ along CH are known from the theoretical potential distribution. The knowledge of the potential gradient $\partial V/\partial r$ along CH compensates for the unknown boundary BG, and it is an easy matter to put values of the potential along BG which give the correct values of $\partial V/\partial r$ along CH in the relaxed mesh. When these values are obtained, the equipotential connecting B to C is found by inverse interpolation along each of the mesh lines. The required profile is then EBLMNCD....

§4. THE FINAL CALCULATION OF THE *n*-VALUES

Since approximations must be made in the design of a profile it is desirable to make a final check on the values of *n*. Two methods, electrolytic tank measurement and magnetic measurement on the actual synchrotron magnet, have been mentioned as being available. The former method, even when a wedge-shaped tank was used to preserve the cylindrical symmetry, did not prove to be sufficiently accurate. The determination of *n* by relaxation methods* seems to have been overlooked. It has the great advantage of being able to take into account the presence of the adjacent exciting coils and the betatron core and of not requiring the manufacture of a magnet or model to test the values of *n*. The relaxation mesh is used to determine the values of the magnetic potential, and from this the field and the *n* values are readily computed numerically by equation (5). The effect of the betatron core and of the exciting coils is readily found by a variation in the boundary values on the relaxation mesh. In this respect it has been found that the exciting coils do not affect the values of *n* appreciably, but that a movement of the betatron core radially outwards may have a considerable effect on the *n* values. This effect is easily assessed since the betatron core is a very suitable boundary for the relaxation mesh.

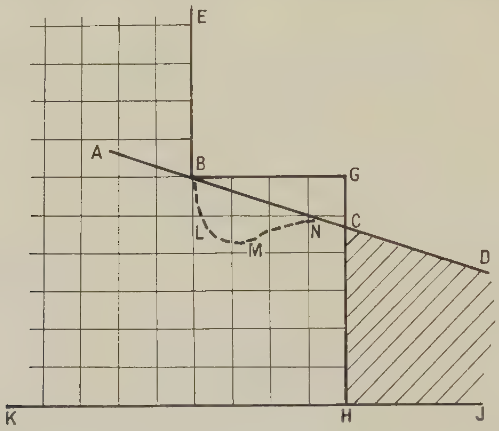

Figure 1. The relaxation mesh for approximating to the extremity of a pole profile.

§5. COMPARISON OF RELAXATION AND EXPERIMENTAL RESULTS

In the case of one pole profile a check on the *n* values obtained by relaxation methods has been made by comparing them with A.C. magnetic measurements made on the actual magnet whose pole profile is shown in Figure 2. In the case of magnetic measurements the *n* values were obtained corresponding to various time intervals after field zero. The ones selected for comparison with theoretical values are those before betatron pole saturation.

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* The methods are described by Fox (1947).

Figure 3 compares graphically values obtained by the two methods for $z=0$ and $z=1{\cdot}5$ cm. respectively. There appears to be reasonable agreement between them particularly with respect to maximum and minimum values. The slight displacement radially between corresponding curves may be due to inaccuracies in the experimental technique, whilst the slight difference in slopes at low radii for $z=0$ may be explained by the effects of pole-packing factors and flux-path reluctance which were not taken into account in the relaxation process.

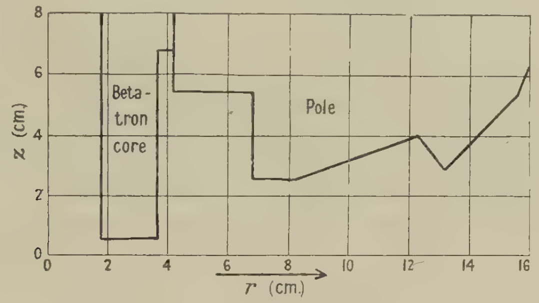

Figure 2. The pole profile used for comparison of relaxation and magnetic measurements.

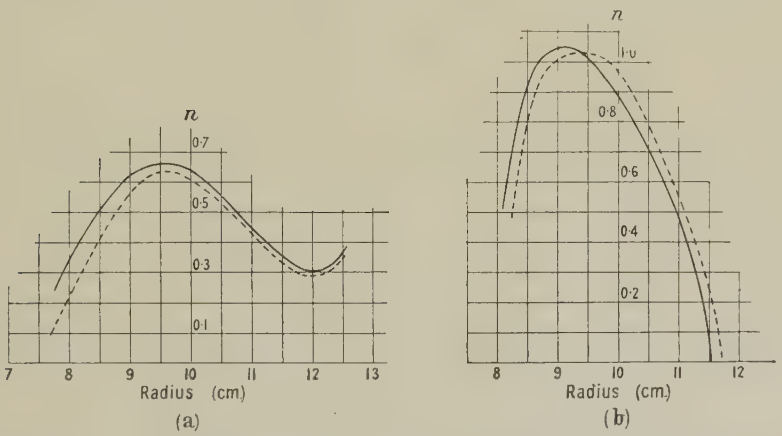

Figure 3. Comparison of relaxation values (continuous lines) and magnetic measurements (broken lines) in the determination of n.
(a) n values on the median plane ($z=0$).
(b) n values on the plane $z=1{\cdot}5$ cm.

A check on the boundary values assumed for the relaxation mesh was made by comparing values of the field H_z obtained by the two methods in the neighbourhood of the poles. It was found that the magnetic measurements at radial distances between 14 cm. and 16 cm. agreed closely with the relaxation values, thus proving that the relaxation boundary conditions were satisfactory.

The relaxation process also enables total flux calculations to be made by a simple summation method and the flux density at any section of the pole to be determined for a given orbit flux density. Values of flux density obtained by this means for a pole profile similar to that in Figure 1 (*b*) of the note by Goward and Wilkins (1948) agreed to within 5% with experimental results. The method has recently been used to modify this profile to that in Figure 2 enabling a 30% increase in maximum orbit flux density to be obtained. This also has been verified by experimental results.

§ 6. PRACTICAL EXAMPLE

As an example of the method of §§ 2–4, a potential distribution which has a value of n within the range $0{\cdot}73 \pm 0{\cdot}08$ over the area $0{\cdot}675 < r < 1{\cdot}325$; $-0{\cdot}2 < z < 0{\cdot}2$ is given by considering only two terms of the series (7), namely,

$$V = -J_0(1{\cdot}12r)\sinh(1{\cdot}12z) - 10Y_0(0{\cdot}26r)\sinh(0{\cdot}26z), \quad \ldots\ldots(11)$$

$J_0(x)$ and $Y_0(x)$ being Bessel functions of the first and second kind respectively, and the radius of the equilibrium orbit again being taken as unit of distance. For an equilibrium orbit of 12 cm. this potential distribution gives a satisfactory n value over a cross section 8 cm. wide and 5 cm. deep.

Any convenient equipotential of (11) can then be taken as the pole profile. In practice it is found that the equipotentials are almost exactly straight lines, to a very high degree of accuracy in the important area, and it has been found convenient to approximate to them by straight lines. The linear approximation to that equipotential of equation (11) which passes through the point $r = 0{\cdot}6154$, $z = 0{\cdot}18$ is

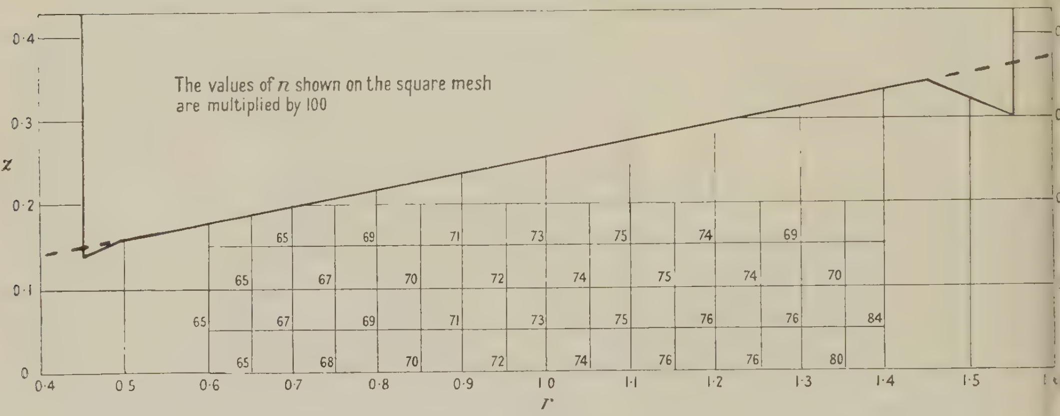

Figure 4. Practical example of the design of a synchrotron pole face showing
(*a*) The scale, taking the equilibrium orbit as unit radius.
(*b*) The lips correcting for fringing.
(*c*) The n values in the area between the poles.

$$z = 0{\cdot}1867r + 0{\cdot}0666, \quad \ldots\ldots(12)$$

and this is a suitable pole profile.

It was required to terminate this profile at $r = 1{\cdot}55$ and $r = 0{\cdot}45$ on the outer and inner edges respectively. The prescribed n values $(0{\cdot}73 \pm 0{\cdot}08)$ were to hold for values of r between 0·675 and 1·325. With these conditions the profile was found to be defined by equation (12) modified by the lips shown in Figure 4, these having been designed by the method of § 3.

Relaxation methods were then employed to assess the final values of n for this profile. The n values are shown in Figure 4 at the corners of a square mesh. It will be observed that they are exactly constant for variation in z only and that they vary radially between 0·65 and 0·80—the prescribed limits.

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REFERENCES

Bartlett, J. H., 1943, *Phys. Rev.*, **64**, 185.
Coggeshall, N. D., and Muskat, M., 1944, *Phys. Rev.*, **66**, 187.
Davy, N., 1948, *Proc. Phys. Soc.*, **60**, 598.
Fox L., 1947, *Proc. Roy. Soc.* A, **190**, 31.
Goward, F. K., and Wilkins, J. J., 1948, *Proc. Phys. Soc.*, **61**, 580.

REVIEWS OF BOOKS

Elementary Nuclear Theory, by H. A. Bethe. Pp. vii + 147. Second Edition. (New York: John Wiley & Sons, Inc.; British Agents: Chapman & Hall, 1948.) 15*s*.

When the reviewer recently went on a trip visiting Universities in England and Scotland he was struck again by the intense interest in nuclear physics among the graduating scientists. No doubt atomic energy with its many problems has created this favourable situation and has led to popularity of work connected with nuclear physics. It is therefore astonishing that the literature on nuclear physics, in so far as up-to-date books are concerned, is extremely unsatisfactory. Though there are some American textbooks, no book has recently been written which covers nuclear physics experimentally or theoretically with any pretensions to completeness. One reason for this state of affairs is, no doubt, the superior attraction of experimental research for the scientist, in contrast to the labour of writing a book which, even though the work is of modest dimensions, must always be a formidable undertaking. To produce a successful book requires, first of all, a very wide knowledge and a thorough penetration into the field, a requirement which already reduces the number of possible authors, especially as most physicists have become specialists within a small field inside the realm of physics. It is necessary that the author can express himself with lucidity, without which the book will, in general, remain on the shelf and not be read, though it may be bought; and if the author succeeds in injecting a dash of artistic brilliance into his representation, it may even be an eminently readable book. Anybody who knows Professor Bethe also knows that he is eminently qualified on both these points. His vast knowledge of quantum mechanics and the experimental and theoretical side of nuclear physics invariably impresses all physicists in scientific discussions. The astonishing clarity of his lectures secures him an enthusiastic audience. Professor Bethe's book on the *Elementary Nuclear Theory* is, since it contains the subject matter of a series of lectures, necessarily very restricted, but it is admirably clear and it contains a great many of those little pointers which are so often left out but which just make it possible for the reader to understand the problems.

The professional mathematical physicist often presents theory in a form which makes it very difficult for the experimental physicist to see the physical basis behind the formulae. Professor Bethe has very successfully avoided this difficulty, at least in the earlier part of the book.

The subject matter is divided into three main sections. First there is a descriptive part where most of the important facts about the properties of nuclei are condensed into 20 pages. Next there is a section concerned with the quantitative theory of nuclear forces. Since the theory of the deuteron may be said to equal in importance that of the hydrogen atom in atomic nuclei, it is treated to a considerable extent on 35 pages. The items included under this heading are: the ground state of the deuteron, the scattering of neutrons by free protons and bound protons, and the photo-effect of the deuteron. The rest of this section is concerned with non-central forces, the saturation of nuclear forces, and a very brief account of the meson theory of nuclear forces. Particularly helpful are the discussions about the interaction in the case of tensor forces, from page 73 onwards, a topic which as a rule considerably frightens the experimental physicist.

These two sections alone make the book worth buying.

The third section (Chapter 1) deals with beta-ray disintegrations, containing many useful numerical data about this field, and a discussion of the Gamow–Teller selection rules which are so important. The compound nucleus is treated in the second chapter of the volume, and a very brief survey is made of the relevant features of Bohr's theory. As an appendix the author has supplied a very useful table of nuclear species.

In spite of the excellence of the book, one aspect of the general treatment of the subject matter has puzzled the reviewer though it is not specific to the book under review. It is this: at the beginning of the book it is shown in great detail how the mass–energy relation in nuclear reactions allows one to calculate the energy reaction, and this is demonstrated with a numerical example. From this one will conclude that the book is meant for real beginners in the field. But on page 50 Pauli spin operators are used, and on page 107 the five types of possible interaction in the beta theory, which are covariant under Lorentz transformations, are given, involving the various Dirac operators. The dilemma in which any author finds himself when he writes such a book becomes very clear here. To be consistent, considerably more explanation would have to be given, which, however, is not nuclear physics but quantum mechanics. I do not know if the person who needs to read the explanations given at the beginning of the book is really able to get to the end of it. I would be surprised if the average engineers and physicists of the General Electric Company of America, before whom these lectures were given, could with a good conscience say that they followed these last-mentioned developments completely. These examples were given more as an added illustration of the difficulties to tbe encountered than as a criticism of this excellent little volume. E. BRETSCHER.

Atomic Energy, by K. K. Darrow. First Edition. Pp. 80. (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948.) $2.

This book contains the substance of four lectures delivered by Dr. Darrow to an audience of non-physicists in 1947. Dr. Darrow, who is the secretary of the American Physical Society, has a well-deserved reputation for the clarity of his expositions, and the present book is a model of how a work of popularization should be written. The treatment is limited to the essentials, and there are no obscure references intelligible only to the initiated. Where accuracy has had to be sacrificed for the sake of simplicity, as with the distinction between mass and weight, this fact is not disguised.

The first lecture introduces the reader to the atomic structure of matter, the nuclear atom, isotopes, and nuclear masses. In the second lecture the equivalence of mass and energy is considered, and the balance of energy and mass in nuclear reactions is discussed with reference to experiments using the Van de Graaff generator and the Wilson cloud chamber. The third lecture deals with some typical nuclear transformations, leading up to the fission process and the possibility of a chain reaction. In the last lecture the method of obtaining a controlled chain reaction in the atomic pile is described, and the uses of the pile for the production of plutonium and radioactive isotopes are briefly considered.

Within its limits, therefore, the book gives an excellent general account of atomic energy, and it should prove especially valuable to those non-scientists and sixth-form students who wish to obtain a fairly simple but reliable and balanced account of the subject.

H. R. ALLAN.

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ABSTRACTS FOR SECTION B

Steam in the Ring Discharge, by G. I. Finch.

ABSTRACT. The behaviour of steam and its decomposition products in the ring discharge has been examined.

Dry hydrogen is not dissociated. The production of atomic hydrogen is dependent upon the presence of steam which dissociates into hydroxyl and atomic hydrogen. A secondary source of atomic hydrogen is then afforded by the interaction of hydroxyl with molecular hydrogen.

The escape from the discharge of atomic hydrogen, a long-lived species, favours the dissociation of steam. Mercury vapour, on the other hand, inhibits the formation of atomic hydrogen and thus leads to a high equilibrium steam concentration.

Unlike dry hydrogen, dry oxygen is dissociated into atoms, but these have a short life as such and recombine in the discharge to form molecular oxygen and ozone.

The reaction mechanisms occurring in the discharge are discussed in the light of spectrographic results.

The Electron Optical Properties of the Focal Isolation β-Ray Spectrometer, by D. K. Butt.

ABSTRACT. An attempt is made to show theoretically how the performance of a lens spectrometer depends upon the degree of spherical aberration present in the lens. In particular, curves of resolving power and transmission are plotted against radius and area of the central lens annulus, for source and counter window on the axis and of equal diameter.

The resolving properties of a circular source and counter window placed at equal distances off the axis are determined. The arrangement is found to be impracticable.

Lastly, an investigation is made into the resolving properties of a line source placed at right angles to the axis of the spectrometer, the image of which is focused into a slit-shaped window in a counter. The arrangement is shown to be advantageous particularly when long solenoids are employed, as in high energy β-spectroscopy.

A Unit-Magnification Optical System for the Attainment of Long Working Distances in Microscopy, by J. Dyson.

ABSTRACT. To fill an application for a microscope with a long working distance, several optical systems are reviewed. Of these, a system giving unit magnification to be used in conjunction with a conventional microscope is selected as suitable. This consists of a spherical mirror used with the object near its centre of curvature.

The aberrations of such a system are discussed and means of correcting them described. Modifications necessary to enable vertical illumination to be used are also described, and photographs taken by means of an experimental system are shown.

Other applications of the same system are suggested and designs given for a miniature system capable of being used on an ordinary microscope, for an immersion objective for " nuclear plate " work and for a water immersion system for biological applications.

The Optical Constants of Thin Metallic Films, by R. Weale.

ABSTRACT. The optical constants of thin films are shown to depend on the thickness of the films due to the variation of the electrical conductivity with thickness. Good and bad conductors both exhibit maxima in the curves relating absorption coefficient and film thickness : in the two groups these peaks are due to two entirely different causes. The mean free paths of the conducting electrons are calculated from data found in the literature.

The Displacement–Frequency Characteristic of Elastically Coupled Mechanical Systems with Two Degrees of Freedom, by D. H. Peirson.

ABSTRACT. The displacement–frequency characteristic of elastically coupled mechanical systems with two degrees of freedom is calculated. Conditions are obtained for independence of deflection and frequency. The practicability of these conditions is considered and a determination made of the extent of the range for displacement independent of frequency.

The relations between the fundamental constants of the systems are illustrated graphically and the application to galvanometer and accelerometer design is discussed.

Note on Volt-Dependent Poisoning Effects in Oxide-Cathode Valves, by G. H. Metson.

ABSTRACT. The high vacuum oxide-cathode valve may show cathode poisoning effects when its electrodes are bombarded by electrons of certain discrete energies. Hamaker, Bruining and Aten describe one such effect occurring at about 10 ev. The author confirms this result and describes two other effects occurring at 5·56 ev. and 15·9 ev. The energies of the three effects can be closely associated with the heats of formation of the chlorides, monoxides and sulphates of barium and strontium. It is concluded that the 5·56 ev. effect is due to bombardment dissociation of the monoxides of barium or strontium, the 10 ev. effect to the chlorides and the 15·9 ev. effect either to the sulphates or to a gaseous ionization phenomenon.

CORRIGENDUM

"Temperature Measurements of Flames containing Incandescent Particles", by H. G. Wolfhard and W. G. Parker (*Proc. Phys. Soc.* B, 1949, **62,** 523).

In the abstract of the above paper in Section A, p. 536, 3 lines from the bottom, *for* melting point *read* boiling point.

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